THE JOURNAL OF PARAPSYCHOLOGY

Volume 14 DECEMBER, 1950 Number 4

Editorial PAGE

Mid-Century Inventory of Parapsychology 227

The Consistent Missing Effect in ESP 244
REMI CADORET AND J. G. PRATT

Electric Shocks for Errors in ESP Card Tests 257
W. A. MCELROY AND WINIFRED R. K. BROWN

Minor Articles and Notes

An Exploratory Study of the Mental Health and Intelligence of ESP Subjects 267
OLIVIA B. RIVERS

Some Exploratory Tests in Dowsing 278
J. B. RHINE

Book Reviews

Ergebnisse Okkulter Forschung by Rudolf Tischner 287
ALFRED WINTERSTEIN

Telepathie und Hellsehen by Alfred Winterstein 289
WILFRIED DAIM

Glossary 291

A SCIENTIFIC QUARTERLY DEALING WITH EXTRASENSORY PERCEPTION, THE PSYCHOKINETIC EFFECT, AND RELATED TOPICS

DUKE UNIVERSITY PRESS
DURHAM, N. C.

The Journal *of* Parapsychology

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This JOURNAL is published on the fifteenth day of March, June, September, and December by Duke University Press, Durham, North Carolina.

Contributions submitted for publication and all editorial communications should be addressed to the Managing Editor, Dorothy H. Pope, College Station, Durham, North Carolina. Correspondence with the editors is advised before submitting articles other than reports of experimentation. Since it does not forward manuscripts by registered mail, the JOURNAL cannot guarantee that they will not be lost in transit, and contributors are urged to keep copies of their papers. All contributions should be typewritten, double spaced. References should be given in the form adopted by the JOURNAL.

Reprints may be ordered when the proof is returned. The JOURNAL will bear one half the cost of reprints up to two hundred copies. Correspondence concerning subscriptions, change of address, back numbers, and other business communications should be addressed to Dorothy H. Pope, College Station, Durham, N. C.

The subscription price is $6.00 a year; single current numbers $1.50. The rate for back volumes is $7.00; for single numbers $1.75. Missing numbers will be supplied free when lost in the mails if written notice is given within one month of the date of issue. All remittances should be made payable to the JOURNAL OF PARAPSYCHOLOGY.

Subscriptions may be obtained in Great Britain through the Cambridge University Press, Bentley House, Euston Road, London, N. W. 1.

Entered as second class matter May 21, 1942, at the post office at Durham, N. C., under the act of March 3, 1879.

DUKE UNIVERSITY PRESS

The Journal *of*
Parapsychology

Volume 14 DECEMBER, 1950 Number 4

EDITORIAL

MID-CENTURY INVENTORY OF PARAPSYCHOLOGY

IT WOULD APPEAR that now is a good time to take stock in parapsychology and ascertain the true extent and value of the holdings. Such a survey is, of course, needed from time to time for the guidance and appraisal of the research program. But there is special reason today for such an inventory, aside from the mere coincidence with the midpoint of the century. Movements are already well under way in the world today which may seriously affect the prospects and possibilities for research in this field, as in all other fields not concerned in an obvious way with war operations. Decisions will have to be made concerning what should be done and how much should be attempted in parapsychology during this period of special handicaps and hardships, and these decisions ought to be made in the light of what has really been accomplished in the field. A general stocktaking should help also to discover how much agreement there is regarding the scope and significance of the findings.

A truly impartial inventory of the state of parapsychology is extremely difficult to make. Even if we were to assume the greatest objectivity of viewpoint on the part of the assessors and the most ruthless intellectual honesty in every judgment reached, the task of appraising so new and unsettled a stock of findings would still be a very trying one. In recent years there have been major shifts

of value in the entire list of items already on the books because of new developments that affect the older ratings. Moreover, standards of valuation have naturally not reached any great degree of unanimity in so pioneer a field as parapsychology. We can at best, therefore, submit a tentative listing and rating of what has been accumulated, hope there will be substantial agreement, and trust that all misjudgments will be corrected.

The stock to be appraised in this survey can all be listed under the general heading of psi phenomena. By that we mean phenomena of personal agency (experience or behavior) that do not admit of physical explanation (speaking, as of course we must do, within the framework of present knowledge). It is this property of being nonphysical that has characterized the phenomena with which parapsychology has dealt throughout its history, and this is the only distinctive property common to them all.

There are two general classes of psi phenomena for which there is acceptably good evidence, extrasensory perception (ESP) and psychokinesis (PK). There may be still other basic classifications, although it is difficult to conceive of any that would not be subordinate to one or both of these two types.

The case for the establishment of ESP is now so strong that it has been a long time since any responsible and informed critic has seriously challenged it. There are indications that a silent resistance to the findings and implications of ESP still prevails, more especially in those psychological circles that are committed to a physicalistic conception of man; but this situation is now more than anything else a case of resistance to information. If there is any ground left for rejecting the conclusions of the psi investigations it has not been made public.

The finality of this rating may be supported by a number of lines of evidence. It is now fifteen years since the publication of the Pearce-Pratt series of ESP tests (at 100 yards distance) in the *first report* of the Duke University studies. No one has yet come forward with a reasonable alternative to extrasensory perception as an explanation of the results obtained. It is ten years since the reporting of the elaborately controlled Pratt-Woodruff series of ESP matching tests. This was an experiment in which there was

a great concentration of precautions against all recognized possibilities of error; yet the experiment led to the conclusion that only extrasensory perception could have produced the results. This series, too, stands uncriticized to date. It is also nearly as long since the publication of Soal's first report on ESP, in which he described an experiment that was also elaborately safeguarded and was buttressed by the observation of many witnesses. Furthermore, Soal's work yielded evidence of ESP in such a way that it came as a complete surprise to the experimenter himself, whose attitude to previous work had been critical.

Such researches, however, constitute only the first line of evidence of the occurrence of ESP. Should the critical analyst ever penetrate this outer defense, he would now find behind it line after line of evidence of lawful character from secondary effects that can be accounted for only by extrasensory perception. Among these further types of confirmatory studies are those which, for example, deal with the personality correlates of ESP, researches in which subjects in ESP tests are separated into high and low scorers on the basis of wholly independent measures of attitude, interest, intelligence, or other qualities of personality. Again, there are numerous studies of position effects that show regular tendencies on the part of subjects to succeed better in ESP at one point or position in the test than another, and these effects produce curves of distribution of hits that are reliable. There are curious effects of other types, too, regular enough to constitute in themselves new lines of research and even to afford independent evidence of ESP. Without further extension of this list, then, it may be said today with complete dispassionateness that to be doubtful of the occurrence of ESP is to be uninformed on the evidence that has been accumulated.

On the PK side of psi there is much less evidence; nevertheless it too seems to be firmly established as a genuine capacity. For one thing, less evidence is needed for PK, coming as it does in the wake of the development of the case for ESP. Many of the statistical problems involved in evaluating the data are the same for both types of psi elements. Furthermore, if we are led to recognize, as we did in ESP, the occurrence of direct psychophysical interaction between subject and object without known bodily intermediation it

is much easier to accept such interaction merely in a reverse direction in the PK process than it was to accept the fact of its occurrence in the ESP work. We are, in fact, prepared by the natural Law of Reaction to *expect* exactly what has been found.

The decision to publish reports of the PK researches in the first place was based primarily on the evidence of widely distributed and highly reliable position effects discovered in the data *years after* most of the original researches had been made. These position effects were mainly represented in the quarter-distribution studies in which it was found that there was a tendency of the subject to make the greatest number of hits in the upper lefthand corner of the record page and to decline in scoring rate downward and to the right across the page. The great significance of the pooled data with regard to this difference left no alternative to the conclusion that the mind of the subject had influenced the falling dice, but had done so in a pattern that came to light only through these later analyses. In addition, the same type of study of hit distribution was made on smaller units of the record page called sets, and this study gave comparable and equally significant results. These two largely independent findings of page and set distribution, along with the original evidence from the score deviations and the fact that in some series the handling of the dice was mechanical, all taken together made a case for PK that no one has succeeded in explaining away by alternative hypotheses.

The work of a number of investigators is involved in these earlier studies. More recently there have been other independent confirmations, one of the more thoroughgoing being the recent one of Dr. R. H. Thouless, about to be published in the *Proceedings of the Society for Psychical Research.*

That psi phenomena of both general classes sometimes occur may then be taken as well established. The relative unassimilability of these findings by the physicalistic psychology of the day has, however, made the task of establishing a recognized case for them much more difficult than it should have been, not only by requiring an excessive amount of evidence, but also by discouraging research by less resolute individuals, and by suppressing publication of important findings

in some instances. Accordingly, the case for psi phenomena is even stronger than the *published* evidence would indicate.

* * * * *

Progress on the subdivision of psi into its types has not advanced so far as the establishment of the occurrence of psi phenomena. The survey of progress in this area of problems cannot be made yet with the same confidence as that with which we concluded the preceding section. In fact this area has in recent years been the scene of a new order of discussion in parapsychology. No longer do the older counterhypotheses of experimental error, statistical inadequacies, selection of evidence, and the like come up for serious consideration; rather the current counterhypotheses today all have to do with the differentiation of types of psi, one from the other. These issues are: whether clairvoyance can successfully be excluded from possible operation in a telepathy test; whether clairvoyance itself has been demonstrated under conditions that excluded the possibility of telepathy; whether precognition could explain the psychokinesis results; and conversely, whether the PK hypothesis may not account for the evidence claimed for precognition. We shall undertake to outline the present state of these issues, but will have to leave the detail of supporting evidence to the original research reports.

The issue over the experimental distinguishability of telepathy and clairvoyance as types of ESP has been a recurrent one since 1934. It is a special aspect of the general question of what range of target (or stimulus) it is possible for ESP to utilize, whether it can directly apprehend a purely mental (whatever that is) state or process on the one hand, and on the other a wholly objective event or situation; or again, whether ESP can operate on a target which does not actually exist until a later time, as in a test of precognition; and there are other possible variations.

The problem of designing a test of telepathy (regarded as a mind-to-mind transfer) that safely excludes clairvoyance has arisen a number of times and caused greater difficulty each time. Now it seems clear that for the present no adequate test for a telepathic type of ESP can be designed, none that completely rules out the possibility of the clairvoyant perception of some objective sign or associated effect (such as postural, vocal, or even nervous action)

that accompanies or follows the sender's thought. The possibility of telepathic ESP must, of course, be kept open and a way of testing for it be sought. The existing evidence of a person-to-person type of transfer of thought by ESP could, as Thouless and Wiesner have pointed out, be explained as a special case either of clairvoyance or of psychokinesis. (That is, the transfer could conceivably be routed either by way of the sender's physical organism or the receiver's.)

On the other hand, much, if not most, of the experimental work carried out under conditions intended to test for clairvoyance does not adequately exclude possible explanation by precognitive telepathy. The ordinary card-calling techniques (BT or DT) are not adequate to test for clairvoyance, since the subject could, by precognitive telepathy, perceive the card order as the experimenter will be recording it later. There is, however, other work that does appear satisfactorily to meet the requirements for the establishment of pure clairvoyance—namely, all those test procedures in which the subject indicated his choice by an act (as in a matching test) which was itself what would have to be precognized. The same act presumably could not serve both as a free choice and as a precognized act. Taking precognition at its intended meaning, foreknowledge of *actual* occurrences (and not of possible but unreal courses of action), we may say that in those experiments in which the *subject's response* (e.g., placing a card opposite a given key card) *has to be made before its results can be precognized,* precognition of the choice could not be used to determine the choice. In other words, if the subject (either in matching a target deck of cards with another deck by the ESP shuffle method, or matching cards against a set of key cards as in screened touch matching) has any true choice in the matter, he has to exercise it *before* a precognitive experience of the consequence is logically possible. If we keep within our present framework of knowledge, there is a causal sequence, a determinism, here which does not allow the subject's use of precognitive telepathy in the placement of the cards. Successful placement of the cards would have to be made through clairvoyance of the actual card order at the time *plus* clairvoyance of the key deck or set of key cards. Precognitive telepathy is possible on the key deck

(or set), but not on the target cards, which have to be placed before they are precognizable via telepathy. This statement holds equally well even if the subject or the experimenter remembers where a given card was placed and later notes what it was.

Thus, if we adhere to the only conception of precognition that makes any sense to our present framework of thought, the subject in these matching tests cannot foresee the order of his own cards through telepathy with the experimenter until after he puts his cards where they are to go, and it is too late then to benefit him; that is, by the time he can precognize them, they are placed. Once their place is fixed in the series of actual and, therefore, precognizable events, the deed is done, the subject has made his response, and clairvoyance is the only way left to account for success in exercising such a choice.[1] (Essentially the same is true of the electrical machine tests of ESP reported by Tyrrell.)

An experiment could be designed that would further simplify the interpretation of the results, if significant results were obtained. All that is needed is a test machine that completely mechanizes the selection of the target, the recording, scoring and counting of hits. The demands to be made on such a machine are considerable, however, and the undertaking should not be entered upon too lightly; but it should be an entirely practicable possibility.

* * * * *

The researches in precognition and PK have developed simultaneously, and quite naturally there was awareness of the need in each type of experiment to guard against the possible operation of the other process. There was, and still is, however, a question of how far it is necessary to go in this safeguarding. For example, in the research in precognition the ESP shuffle experiments showed at an early stage that with hand shuffling it was possible for clairvoyance to influence the shuffling; and as a result, a mechanical shuffler was introduced. Then after some preliminary testing, the possibility of influencing the shuffling machine psychokinetically was recognized as justifying other methods of randomizing the cards.

[1] Some readers may wish to review the discussion of these problems in further detail in the following articles by J. B. Rhine in the *Journal of Parapsychology:* "Telepathy and Clairvoyance Reconsidered," September, 1945; "Precognition Reconsidered," December, 1945; and "A Digest and Discussion of Some Comments on 'Telepathy and Clairvoyance Reconsidered,'" March, 1946.

Among the other measures adopted was the use of official daily temperature readings, the figures to be used in a simple computation that would determine the deck of cards to be cut. It was recognized, though regarded as unlikely, that if the temperature (or for that matter the thermometer making the record) could be influenced psychokinetically by the experimenter, this precaution would not be sufficient. A few students of parapsychology have not been satisfied with the adequacy of this precaution, but thus far none has made any experimental attempt to improve on the case as it stands. It is not difficult now to design a more clearcut method of excluding PK and doubtless further work will carry the issue to a point of further refinement. It would seem adequate to introduce into the procedure of cutting the deck of cards a routine but highly complex mathematical computation such as has to be carried out on an electrical computing machine, something that is beyond the experimenter's capacity even if he presumably has everything else that is needed.

It should not be supposed, however, that the present status of the case for precognition depends simply upon the rejection of the alternative possibility that the experimenter may have been able to influence the thermometer or the weather to the perfect degree required. He would also have had, as a second step, to have some clairvoyant awareness of the order of the cards in the decks, and, third, an adequate amount of clairvoyant awareness of the subject's record sheets which he had not seen. Fourth, he would need to locate this temperature recording station clairvoyantly and, fifth, unconsciously compute the temperature maxima and minima that would combine to give the favorable matching desired between the various decks of cards to be matched on that day with the particular record sheets appropriate to each deck. He then would have to produce the exact PK effect on the thermometer required. It need hardly be said that all this added together makes a formidable combination and it is doubtful if anyone could really suspect that the experimenter would and could go through all this procedure unconsciously (and, of course, without intending to do it, since he would only be deceiving himself). At the same time, if a further modification of method could entirely eliminate a remaining question, that would always be

the preferred course to take. Precognition especially needs the fullest possible examination.

On the PK side of the relationship, there is the question as to whether precognition of the future die throwings in the test may not guide the experimenter or subject in selecting the target face or combination of faces for a given run or set or series. The hypothesis of precognition was not important as an alternative until the work in PK was far advanced; not, in fact, until the other more plausible counterhypotheses had been eliminated, and a strong case for PK established. This was after the discovery of the elaborate position effects represented in the quarter-distribution studies. Once these position effects were appreciated, however, it did not seem reasonable to think that a target face, or combination, had been selected by precognition that would give *both* a significant deviation on the target and the significant and elaborate position effects that such a series as the Reeves High-Dice and Low-Dice Series shows. It still does not. Thus precognition as an alternative possibility was recognized but not taken seriously. There were, however, experiments such as the help-hinder studies of Humphrey and the disc experiments of McMahan in which the target faces were incidentally chosen by the throw of a die. More recently the dice experiments of Thouless have covered this point thoroughly. So PK stands pretty clear of precognition as a counterhypothesis.

The present state, then, of what might be termed "*intra*-parapsychological" relations summarizes as follows: Psi activity, representing psychophysical interaction between subject and object without sensorimotor intermediation, has been amply demonstrated to occur. Under conditions that allow no reasonable doubt both cognitive and kinetic psi effects have been established as genuine occurrences. The inventory yields quite a clear case for both PK and ESP. Breaking the latter down into its types, we may say that the case for clairvoyance is fairly good, as is that for precognitive clairvoyance—though there are possibilities for improvement in both. There is at present no clear case for telepathy (which, of course, leaves the question open).

* * * * *

We turn now to the consideration of *extra*-parapsychological

relationships, that is, to the ways in which psi phenomena relate to certain neighboring sciences with which we are more familiar—to physics, psychology, and biology. Psi phenomena are, of course, psychological; and as functions of living things, they are, in a general way, biological too. Moreover, in their interaction with material objects they become identifiable as psychophysical manifestations as well. These relationships are all part of the total stock we are attempting to list—an essential part of the inventory.

Unquestionably, the most significant relationship in parapsychology is that concerning physics. By definition parapsychological phenomena do not yield to physical explanation. If ESP were found to be based on a radar type of process, or if PK were found to utilize a special form of gravitational energy, both phenomena would obviously be taken over by physics; and they would just as obviously no longer be of interest to parapsychology.

It is important, then, to know what experimental confirmation there is for this extraphysical character of psi. By this time the evidence is sufficiently good to warrant the conclusion that distances of several hundreds of miles, even of thousands, do not show any discoverable relationship to success in ESP tests. These distances are large enough at least for a gross comparison, and in view of the existence of natural barriers in most of the long-distance tests, it would definitely appear that nothing within the range of energies at present known to physics can be responsible for psi manifestations.

Probably the most conclusive type of evidence of the nonphysical character of psi, however, is that yielded by the precognition experiments. We have already reviewed the case for precognition, rating it as a moderately good one but with improvement still possible on the point of further safeguards against PK as an alternative hypothesis. There is, of course, no physical explanation for precognition.

The PK experiments, too, contribute to the case for the independence of psi phenomena from physical law. The main contribution involves a comparison of different sizes of dice to determine whether PK is affected by mass. Thus far no reliable relationship has been found between size of dice and rate of scoring. The findings suggest, rather, that the subject's volitional attitudes are factors of greatest importance.

Among other physical tests there have been comparisons of the number of dice thrown at a time, of different physical shapes of the dice, and of distance between the dice and the subject. These have all failed to show any reliable relation of PK results to the physical conditions of the test. The results of the PK comparisons agree, then, with those of the ESP researches that deal with physical conditions. Wherever the question of physical relations has been brought to test, the results indicate that psi is not a physical type of process. It is psychological.

* * * * *

We turn now to psychology and to the question of where parapsychology seems to fit into this field. There is only an artificial boundary between parapsychology and psychology; parapsychology's domain consists of those phenomena of psychology that more sharply and clearly challenge physical explanation. We cannot tell about memory or emotion, for example; but precognition offers a decisive case against physical explanation.

Psychologically, the whole range of psi phenomena are at once classifiable on another basis as well. They are all unconscious in their operation. This is one of the best established characteristics. This unconsciousness of psi operation is responsible for a number of unusual effects not common to conscious functions; for example, there is the well-established effect some subjects show of avoiding or rejecting the stimulus and thus producing negative deviations in scoring when positive deviations are desired. This rejection may either be due to unconscious negativism, or it can also be caused by a persistent error in ESP judgment ("bad aim"). The well-known effect known as displacement, or the hitting of the neighboring target in a run of trials, is another result of the unconsciousness of psi. The fact that ESP is an unconscious process too furnishes a ready explanation for a somewhat similar phenomenon reported in this issue of the JOURNAL—that of consistent missing, or the extrachance calling of one symbol in a consistently erroneous way (for example, a circle for a square).

One of the most reliable and general psi effects resulting from the unconsciousness of the ability is the tendency to position effects. Of these there are a number of types, most of them declines in scor-

ing rate in the column or set or page or session. This decline may even extend over the whole chronology of the series of tests. Other variations of these position effects are already on record, but the full extent is probably not yet known. These declines could hardly be expected to be so general a feature in a conscious process. As it is, since the subject cannot tell when he is doing well or poorly he cannot make the mental adjustments that would correct his approach, as he would do when he knew he was failing.

The question whether psi phenomena are *necessarily* unconscious has not yet had even a tentative answer. There are many suggestive indications, especially from spontaneous psi experiences, that a subject may have at times a reliable conviction of the rightness of his response or the validity of his psi experience. Just how this experience of conviction is related to the actual psi process itself is a question on which we hope to learn something from the case studies themselves in the future.

Another conspicuous relation of psi to psychology is its contrast to the sensorimotor system. The sensorimotor system is of course the person's primary medium of interaction with the physical environment. It is the contrast of psi processes with those of the sensorimotor functions that gives them their significance. The experiencing subject, in having some kind of interaction with the world of objects without the intermediation of the sensorimotor system and without the limitation which the physical world places upon that system, is demonstrating to psychology a world of peculiarly psychological reality which that science has been thus far unable to appreciate properly.

The *contrast* of parapsychology to the currently conventional field of psychology, however, ends with the sensorimotor functions. What we have learned about ESP thus far harmonizes well with other types of cognitive judgment, and this means that the judgmental action in the ESP test experience is probably common to all judgmental activity. The U-curves of performance, the effects of terminal salience, the influence of various motivational factors such as reward, boredom, challenge, and the effect of narcotic and stimulant drugs upon psi ability are all in fair agreement with the

findings reported in connection with comparable cognitive judgment in other kinds of tests.

Although still in the relatively exploratory stages, the studies of the relation of personality characteristics to performance in psi tests have gone far enough that certain significant relationships have been found. It appears to be pretty definite now that much of the difficulty of interpretation lies in the fact that the personality studies of psi are dealing with at least two factors, one of them the amount of ESP ability itself, and another a "sign factor" (or perhaps a group of factors) that determines whether the response will be a positive or a negative one; that is, whether it will tend to hit the target or avoid it. Working under the cover of unconsciousness these factors defy easy analysis as, of course, has been discovered with other psychological studies of unconscious operations. It is fairly certain now that some of the personality studies have been separating subjects, not on the basis of high and low ESP capacities, but rather on the basis of their tendency toward positive or negative deviation in the scoring (the effect of the sign factor). We could with reasonable safety list another factor (or set of factors) as responsible for the decline effects which are now a well established feature of psi test results. However, recognition of these possibilities is in itself a point of progress, and new researches will follow to explore further the part these secondary factors play.

One of the negative psychological observations of great significance to psi research is the fact that psi capacity has not been tied up with any exceptional personality deviation. We have gone far enough to be reasonably certain that there is no close association of psi with any familiar kind of abnormality or subnormality. It is even conceivable that all the personality correlates thus far dealt with could be related to something secondary to the functioning of psi itself, to traits or states that inhibit or qualify the operation or expression of psi ability. Some of these correlates are clearly of this secondary type. Some, however, could, insofar as we yet know, be either of that type or could be more primarily related to psi function itself.

It must be remembered, of course, that in the area of personality correlates we are dealing with the most difficult area of conventional

psychology itself; one in which progress has been relatively slight, judging by the standards held in parapsychology. As it is, we are handicapped for the lack of psychological tools to borrow, and our production must be judged accordingly.

* * * * *

Finally, and with somewhat less confidence, we turn to the relation of parapsychology to biology. If psi functions are psychological and yet nonphysical realities it is important for many reasons to inquire into their biological relationship. Our listing on this line still shows little beyond a series of unanswered questions, but most advances begin that way; it is a good beginning that these questions are recognized as worth while and, in some cases, are already under investigation. It is therefore more than a mere empty recital to list them in the inventory.

Since some psi phenomena are accepted as natural occurrences happening to living persons, must we not expect to find some hereditary basis for the capacity? Otherwise there would be no way of accounting for its appearance in the individual. Again, if it is a human ability, it must have been acquired at some point in the evolution of the species. Was the evolutionary origin of psi early or late in the evolutionary history of life? That question raises many others, such as that of how widespread psi is among other species and, again, that of the biological utility and survival value of psi capacity. There is no end to the chain of questions raised, and we can only select samples arbitrarily. (For instance, do the psi processes have organic equivalents to the receptor and effector mechanisms of the sensorimotor system? Is there any evidence of a localization of ESP reception or of PK action in the organism? Has any sign of localization of psi function in the central nervous system been found? Finally, to choose only one more example, is there any indication whatever of any specific bodily activity accompanying the operation of psi, or, on the other hand, of psi functioning with clear independence of organic accompaniment?) On all these great issues, however, stock is still being awaited.

There is only one biological question to which some advance may be credited, namely, the very fundamental problem of whether personality is in any way extrabiological. This issue is, of course, the

classic one of whether there is any part of personality that can survive the death of the body. It has been carried forward somewhat toward eventual solution in a number of respects, even though it is far from solved as yet.

First, it is an important step to recognize that this question can be successfully investigated only after certain prior studies are made on the nature of the personality of the *living* individual. Second, there has been a fair amount of development in methods of objective evaluation of the verbal responses of mediums. And, third, comparatively good progress has been made in rounding out the kind of counterhypotheses which evidence for spirit survival has to satisfy. The limiting factor now in the research on this problem is ignorance of the psychology of personality. There is simply too little known of what there really is and can reliably be measured in the living person to permit the design of a crucial experiment as to whether some part of the whole could operate separately from the rest. There is much less confusion, however, on the matter of the survival problem than ever before. It is now known, at least in a general way, how the question will have to be approached if a reliable answer is to be found.

* * * * *

A little stocktaking is in order on certain practical problems concerning psi abilities. There has been great difficulty, as is well known, in obtaining evidence of psi abilities, largely because they function on the unconscious level entirely; accordingly it is not surprising that practical utilization is still beyond reach.

On the experimental side there is a corresponding question of whether "repeatable experiments" are possible with psi. Repeatability, however, should not be expected in scientific experiments except in relatively simple fields or in well-developed ones in which we know all the variables and can readily control them in the test situation. The psi explorer is still something of a naturalist, and to require the naturalist on each occasion to capture an identical specimen in the same place repeatedly before the first one would be considered worth looking at would not help him to advance his field.

Both experimental control and practical use would obviously be facilitated if some way could be found to by-pass the handicap aris-

ing from the unconscious operation of psi. Some attempts have been made to explore different possibilities of achieving this objective, and others are being tried. Some of them have taken the form of utilizing automatisms (automatic writing, the divining rod, the pendulum) to elude the inhibitions arising because the process is unconscious. Others have tried different routines of conscious response such as various modes of indicating and registering the response (pointing, placing cards, pressing buttons or keys, making freehand drawings and the like). Enough has been done at least to warrant the tentative conclusion that there is no easily encountered detour around this roadblock.

Some encouragement regarding the possibility of controlling psi functions can be gained from the study of spontaneous psi experiences; but, on the other hand, it may be that the very nonphysical nature of psi phenomena, as now established, forever bars it from *conscious* control. Consciousness itself, for all that is known about it, *may* be limited to sensory interaction with physical stimuli, and the derivatives of sensation (such as memory and imagination). Extrasensory perception may, by the very nature of the case, not lie within the range of conscious experience, even while its cognitive content may be at least partially convertible to awareness.

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Inventories such as this are, of course, quite arbitrary in their evaluations, and relative in their value. They are nevertheless needed for the making of decisions on policy and practice in the research. There is a *special* sense, however, in which this evaluation is necessarily arbitrary and limited. Facts of the kind listed above can only be properly evaluated by some sort of market price, and what they will bring, by what can be done with them. On the shelf or in the abstract, they have no value. Or to the customer who has no use for them or does not know what can be done with them, their real worth is zero. It is as important, then, as anything else that can possibly be related to these facts that their appraisal be made in terms of their relevance to the larger problems and uses for which they were intended by those who produced and collected them.

We have already, from time to time, in this editorial section attempted to draw attention to the larger range of significance which

we have been able to see in the findings from the experimental data. We have discussed their significance for religion, for government, for ethics, for psychiatry, for general psychology, for biology, and for some other less general areas of interest; and even this broad labeling of the meaningfulness of parapsychology's stock is far from complete. This frame of reference should be kept in mind as we appraise the value of the listed findings, for it is only as this fact-plus-meaning relationship becomes more clear and more effectively appreciated that the products which the field has to offer can be brought forth from their obscurity. These larger valuations, then, are a significant part of the total stocktaking on the psi investigations. In fact, the inventory would mean little without them.

J. B. R.

THE CONSISTENT MISSING EFFECT IN ESP

By Remi Cadoret and J. G. Pratt[1]

ABSTRACT: This report deals with the consistent missing effect; that is, with the tendency of the subject in an ESP test to mistake a given symbol for a certain other symbol to a greater-than-chance degree when making his calls. The occurrence of consistent missing in a subject's data, then, would mean that his responses were related in some psychological way to the target symbols at which they were aimed even though the calls did not match the targets.

From the available data, four series were considered acceptable as evidence of consistent missing. When these series were examined individually, none gave statistically significant results by itself; but when they were evaluated as a group, the results were of an order which would be expected to occur by chance about once in 200 such investigations.

A method for evaluating consistent missing is given in the Appendix.

Mr. Cadoret, a medical student at Yale University, was Research Assistant at the Parapsychology Laboratory when this work was done.—Ed.

Introduction

In most of the early studies of ESP, interest attached mainly to the scoring of *hits* on the direct targets, the ones at which the subject was aiming his calls. In recent years, however, research attention has been directed toward a wider range of ESP scoring. It has been found, for example, that some subjects' calls are related to cards other than those at which they were consciously aimed. The displacement effect, as this *temporal* misdirection of ESP has been named, is generally most pronounced when the calls are compared with the targets coming just one step before and one step after the cards the subject was trying to identify. Just as in the case of scoring on the direct target, ESP displacement has been found to be significant in some instances because there were *more* hits than could reasonably be ascribed to chance coincidence and in others because avoidance of the displacement targets had occurred, as indicated by the fact that there were significantly *fewer* hits than expected.

If the subject could only know when he makes an ESP hit, he

[1] We wish to acknowledge our indebtedness to Dr. T. N. E. Greville for his help on the statistical problems encountered in this investigation.

would be able to direct all his scoring onto the target at which he is aiming. The indirect forms of scoring that have been discovered, such as displacement, all arise from the fact that the psi processes do not operate on a conscious level. It is reasonable to expect that this "unconsciousness" characteristic may lead to still other ways of misdirecting the ESP process, and the discovery and investigation of other forms of ESP functioning will be important steps in the advance of knowledge regarding the nature of the psi processes.

The present report deals with another relationship between the subject's calls and the target cards which shows the unconscious operation of ESP. This is the consistent missing effect, or the tendency on the part of the subject in ESP tests to mistake a given symbol for another to a significant extent. The occurrence of consistent missing in a subject's records would mean that some of his responses were related to the target symbols at which he was aiming even though the calls did not match the cards. For example, when the target is *square,* the subject might show a tendency to call *circle.* This problem of whether "consistent missing" occurs arose while we were making a study of the displacement effect and at a time when we were interested in finding other kinds of misdirection of ESP comparable to those already known to occur. We have examined the records of a number of ESP subjects for the consistent missing effect and evidence has been found that this indirect form of ESP scoring does occur. This paper will present the investigation in full up to the time of preparing the present report.

In this investigation we were interested solely in a possible connection between a call and its target symbol when the two were not the same; that is, when the trial seemed to be a miss. To analyze ESP data for consistent missing, it is necessary to know the number of times each symbol was called for each of the other four card symbols and to evaluate these frequencies against what would be expected from chance. This made it necessary to develop a method of evaluating the misses that was independent of the number of hits. A solution to this statistical problem was found which made it possible to apply a chi square test to the *misses* while disregarding the *hits.* The method of evaluation, which involves an unusual application of chi square, is described in the Appendix.

Results

The records examined for consistent missing represent the work of five separate ESP subjects, four of whom contributed one series each and the fifth, four distinct experimental series, a total of eight experimental series. We were influenced in the selection of data to be studied by the following considerations. First, we looked for subjects who had scored significantly above chance, as we wanted a reasonable assurance at the start that ESP was present in the data. Second, we required a large number of runs by each subject so that a weak consistent missing effect might show up by the method of evaluation. Third, it was necessary, for reasons to be explained later, to use subjects who did not show any displacement tendency. When these criteria of selection are applied, there are not so many series available for consistent missing analysis as one might at first expect. We do not claim, however, that the present investigation has covered all the suitable data on record.

Since the hypothesis of consistent missing involved the suggestion that different subjects might have different habits of making wrong associations of calls and cards, it was not considered valid to pool the data of more than one subject in a single analysis. Similarly, we avoided pooling separate series by the same subject when a change of conditions was involved, as it was possible that evidence for consistent missing might be obscured by changes in the patterns of association. A summary of the general findings is presented in Table 1.

The first data selected for analysis were those of the Pearce-Pratt Distance Series, since these records had previously been published in the form of a 5 x 5 table showing the frequency with which each call symbol was given for each of the five card symbols (2). When the appropriate chi square test was applied to the misses alone, this series gave no evidence of consistent missing ($\Sigma\chi^2 = 12.75$; 11 degrees of freedom; $P = .3$).

Since at this stage of the investigation we were simply exploring to see whether any ESP data showed evidence of consistent missing, the chance results obtained from the Pearce work raised a question as to whether this effect would be most likely to occur under the conditions of that test. This was a series in which the subject made

Table 1

TOTAL CONSISTENT MISSING RESULTS OF TEN ESP SERIES

Subject	Experimental Series	Conditions*	Runs	Hits	Consistent Missing Σx^2	P
1. Pearce	Pearce-Pratt (2)	Distance Clairvoyance	74	568	12.75	.3
2. D.L.	Carpenter & Phalen (1) Series 2	Clair., colors; subject holding cards, unscreened; calling top one	140	997	124.24	10^{-28}
3. G.	Carpenter & Phalen (1) Series 1	Clairvoyance; subject holding cards, unscreened; calling bottom one	60	450	27.83	.004
4. G.	Carpenter & Phalen (1) Series 5	Unscreened DT, colors	100	612	9.35	.6
5. G.	Carpenter & Phalen (1) Series 6	Unscreened DT	100	605	16.84	.1
6. G.	Carpenter & Phalen (1) Series 7	Screened DT, colors	100	600	22.47	.02
7. Riess	Riess (3)	Distance GESP	74	1349	17.13	.1
8. E.F.	Cadoret (New data)	Screened DT	105	552	21.21	.03

*ESP cards were used except where colors are indicated.

only one call per minute and only two runs at each session, and the series of 74 runs stretched over a period of several months. Such a long drawn-out series would allow wide opportunity for the subject to change his habits of associating call and card symbols; and if such changes occurred, tendencies to make certain wrong associations in one part of the series might be obscured by different habits of confusing the symbols in another part. In order to test the consistent missing hypothesis under what seemed to be the most favorable psychological conditions, therefore, we decided to examine next some ESP records in which the subject had done a large number of runs within a short time.

The subjects selected as best meeting these conditions were from the Carpenter and Phalen experiments (1). These records were available to us from having been used just previously in another investigation, and some of these results met the general requirements for selection for a consistent missing analysis: large individual series, significant scoring, and no evidence of displacement. When we took as a further condition of selection a large number of runs with a minimum lapse of time, the data of the Carpenter and Phalen experiment which were best qualified were not those from the series with the best ESP test conditions. As we were interested in seeing whether any evidence of consistent missing could be found under the most favorable conditions for this hypothesis, we decided to examine first the series which seemed to be the most likely to contain evidence of the effect.

Following this procedure, we selected for analysis three sessions of subject D. L., comprising 140 runs, from Series 2, and a highly significant consistent missing result was obtained ($\Sigma\, x^2 = 124.24$; 11 d.f.; $P = 10^{-20}$). The targets in this series were shuffled cards, each having on its face one of five colors: red, green, blue, white, and yellow. A pack of 25 of these cards, five of each color, was shuffled by the experimenter and spread out in a single row face down on the table. The subject then moved down the row of cards and attempted to call the color on the face of each, which he did to a highly significant degree But the analysis of the wrong responses showed that "consistent missing" undoubtedly occurred also. In spite of the fact that the subject was not consciously motivated to score in any such indirect manner, we recognized that the conditions of the test did not make it possible to distinguish conclusively between ESP and sensory cues as the explanation of the consistent missing evidence. For a crucial test, we had to turn to other subjects, as there were no data from D.L. obtained under adequate ESP test conditions.

One subject, G., from the Carpenter and Phalen experiments, made a significant number of hits under conditions that were adequate for testing ESP. This was Series 7 in the original report. In addition to analyzing this one series for consistent missing, however, we also evaluated the results of three other G. series to get a

comparison of the effect for one subject over several series. The analyses of the four sets of data by subject G. are presented in the order in which the series were described in the original report.

The consistent missing analysis of Series 1 (60 runs) by subject G., which was based upon the use of ESP symbols, gave a total chi square of 27.83 (11 d.f.; P = .004). In this series G. held the shuffled deck in his hand. He called the bottom card and laid it aside without looking at it. His calls were recorded by an experimenter and checked at the end of the run of 25 trials.

Another evaluation of records by subject G. was done upon Series 5, data in which the colored cards were used as targets. This series consisted of 100 runs in which the DT condition was used, though the cards were not screened from the subject's view. Under these conditions the chi square for consistent missing was 9.35 (11 d.f.), which is quite insignificant.

A third set of G.'s records (Series 6) was like the second, with the exception that ESP cards were used. These data gave a chi square of 16.84 (11 d.f.) with P = .1.

The fourth series of subject G. to be analyzed for consistent missing was the one which was reported as having adequate conditions as a test of the ESP hypothesis—Series 7. This was a screened DT test of 100 runs with colored cards in which the subject scored a deviation of 100 hits on the target. The consistent missing evaluation gave a chi square of 22.47 (11 d.f.) with P = .02.

Encouraged by these results, we proceeded to examine for the consistent missing effect two other series which provided suitable data for this analysis and in which the conditions were adequate to rule out sensory cues. The first of these was the Riess Series A (3). As the experimental conditions here were similar to those of the Pearce-Pratt Series, the Riess data were not selected as a series which seemed especially favorable to the consistent missing hypothesis, but as a series that was acceptable on all the other criteria. This work gave a chi square for consistent missing of 17.13 (11 d.f.), which is not significant when considered alone (P = .1). The second series comprised 105 runs of clairvoyance tests conducted under screened DT conditions. These were runs collected by R.C. in the Parapsychology Laboratory from subject E.F. Although the

rate of hitting on the target was not significant, we decided nevertheless to analyze the data for consistent missing. The chi square was 21.21 (11 d.f.; P = .03).

In order to make a rigorous test of consistent missing as an ESP effect, it is necessary to consider only the results from series in which the cards were screened from the subject's sight. This criterion is met by four of the series shown in Table 1, items 1, 6, 7, and 8. A test of whether these four series in combination show any evidence of the consistent missing effect may be obtained by summing the chi squares and their degrees of freedom for the four separate analyses. The result is a combined consistent missing chi square of 73.56 (44 d.f.), which is significant (P = .005).

The unscreened DT condition provides reasonably adequate precautions against sensory cues, and the evaluation might justifiably be extended to include items 4 and 5 as well. With these two series added, the total consistent missing results for all the data obtained under adequate conditions is still significant ($\Sigma \chi^2 = 99.75$; 66 d.f.; P = .008).

If either or both of the other two series were to be considered along with the others, the significance of the total results would be increased. There can be no question, therefore, of our having made an unwarranted selection of data to favor the consistent missing hypothesis when we consider as crucial only those series in which adequate conditions were used.

Discussion

In the series that gave the most significant chi square, the D.L. series of the Carpenter and Phalen experiments, the fact that the backs of the cards were exposed to the subject at the time he made his calls suggests that some similarity between the cards may have provided the basis of the subject's tendency to miss consistently. But there are indications that the matter may not be adequately explained by the simple assumption that some of the cards looked alike and were therefore confused. We examined the subject's results to see what calls and cards were being consistently confused in order that we might determine whether the meaningful mistakes were such as would be expected if the subject simply had difficulty in distinguishing between certain cards. They were not. The re-

sults showed that he called the red cards white, but that he did not call the white cards red. If the red and white cards had been hard to distinguish, whether on an ESP or on a sensory basis, he should have called too many of each for the other. Again, he tended to call the blue cards red, but he did not call the red cards blue. As we have said before, this series is inconclusive as evidence of ESP and therefore by itself it could not be considered as establishing the hypothesis that consistent missing by means of ESP occurs. However, the present study provides evidence from data taken under adequate conditions that consistent missing is a genuine effect. If this finding is confirmed by later studies, the D.L. series, as one which may show the effect to a remarkable degree, may help to suggest fruitful hypotheses about the nature of the psychological process involved. For this reason we have felt that it is worth while to put this series on record, and the detailed chi-square analysis of the D.L. data was therefore selected for the illustration of the evaluative procedure in the Appendix.

The four series with adequate conditions that are described in the present study had such a slight degree of consistent missing that, although the total effect was significant, it was not sufficiently concentrated to indicate what wrong associations might be responsible. In general, however, we have the impression that the tendency shown in the D.L. data for the more significant relations of calls and cards not to be reciprocal was true throughout the data. This observation can be offered only as a suggestion on the basis of the present investigation, but it is something to be checked further as opportunity is found to extend the investigation to other series.

At one point in the present investigation we considered the advisability of evaluating for consistent missing the data of subjects who scored significantly on one or more displacement targets as well as on the direct target. After further study of the statistical problems involved, however, we concluded that this could not legitimately be done, as displacement coupled with nonrandom sequences in the subject's series of calls might produce consistent missing as an artifact. Suppose, for example, that a subject shows a highly significant tendency to call star when this is the target one step ahead, and that he also has the tendency to call cross after star. In a

tabulation of his data for consistent missing this subject would seem to associate the call, cross, with the target, star. At the present time we know of no valid measure of consistent missing as an independent effect in data that give significant evidence for displacement. If a satisfactory method of separating the two effects can be found, ESP data that show the displacement effect may also be studied for evidence of consistent missing.

The eight series surveyed in this investigation have all been checked for evidence of displacement at least to the extent of comparing each call with the two cards before and the two cards after the direct target, and all of them gave only chance displacement results. (There is no evidence that subjects who do not show displacement to these cards near the direct target score significantly on cards still farther away. For this reason we did not consider it necessary to check displacement beyond the second card away.) The series in this report all qualified, therefore, as data which could be tested for consistent missing as an effect independent of the displacement phenomenon. Series of ESP data that were *known* to show significant evidence of displacement were excluded, as were data on matching tests, which could not be checked for displacement because the order of calls was not known.

With the exception of the subject E.F., all of the records analyzed for consistent missing had direct target scores that were significantly above chance expectation. It is conceivable that one reason why some subjects do not score above chance in an ESP test is that they are consistently misnaming the symbols. The E.F. series presented an opportunity to see whether a subject who scored only a chance number of hits on the direct target showed any difference in the evidence of consistent missing when the runs were grouped according to run scores.

The runs of the E.F. series were divided into two groups: those with scores of six and above on the one hand, and scores of five and below on the other. Each of these score level groups was then analyzed for consistent missing. For the high level group the total chi square was 26.53 (11 d.f.; P = .005); for the low score group it was 7.95 (11 d.f.; P = .7). While this preliminary study can be considered as only of suggestive value, the results indicate that

E.F. showed a stronger tendency toward consistent missing when she was scoring *above* the expected average number of hits on the target. As all of the other subjects studied were successful in scoring significantly high in their runs taken as a whole, the findings indicate that, contrary to the above suggestion, the consistent missing effect is stronger when the subject is succeeding to a considerable degree in naming the targets correctly. It is evident, of course, that a subject who was completely successful in terms of his conscious aim would not demonstrate consistent missing or any other of the secondary ESP effects. As we suggested at the beginning of this paper, consistent missing is only one of the indirect ways in which ESP operates that are traceable to the fact that the subject does not have direct conscious control over the use of his psi abilities.

APPENDIX

Method of Evaluation

The need for some way to test the consistent missing hypothesis led to the suggestion that a chi-square test might be applied to the *misses* alone; that is, to a 5 x 5 contingency table in which the observed hits shown on the diagonal are disregarded. But the evaluation of a 5 x 5 contingency table with the diagonal cells "deleted" presents a novel statistical problem. With the column and row totals of the non-diagonal cells fixed as the sums of the observed values, what is the theoretical expectation for each non-diagonal cell? If these expectations can be obtained, we can compute the chi square for the misses of the table and so determine whether there is evidence for consistent missing. The difficulty, however, lies in the problem of how to get the expected values for the non-diagonal cells alone.

The appropriate theoretical expectation for a particular non-diagonal cell would be obtained by considering for the "miss" cells of the table every possible set of numbers which gives the observed non-diagonal column and row totals, in each case weighting the frequency in the particular cell in question by the probability that the given set of numbers will occur in the "miss" cells if it is known that the numbers in the diagonal cells are those observed, and summing these products. This exact method is not feasible in actual

practice as the work involved is prohibitive. A close approximation to the desired theoretical frequencies may be obtained by inserting on the diagonal the set of figures which would be the expected values in a complete 5 x 5 table, if the observed misses had occurred while the subject was making exactly the most likely chance number of hits on each target symbol. This "neutral" diagonal would contribute nothing to the chi square, yet it would enable us to obtain expectations in the other cells adding up to the marginal totals.[2]

The desired values for this neutral diagonal are expressible algebraically as a set of five simultaneous quadratic equations. The algebraic solution, however, proved impractical, and a method of successive approximations suggested by our colleague, Esther Bond Foster, has been applied. This method consists simply of solving for expected values on the diagonal on the assumption that the marginal totals (those for the misses) actually included the deleted diagonal. The "expected" values so obtained for the diagonal cells are added to the marginal totals, and a second, closer approximation is then made. This process is repeated until the diagonal values show no further appreciable change.

Once the expected values of the diagonal cells are known, the expectations in the non-diagonal cells (misses) may be obtained in the usual manner by using marginal totals that include the values of the neutral diagonal. These expected values for the non-diagonal cells add up to the original marginal totals of misses. As an example of this neutral diagonal method the contingency table of the D.L. series evaluated for consistent missing is shown in Table 2.

[2] That this procedure would give approximately the values desired may be seen as follows. T. W. Anderson has shown (*Annals of Mathematical Statistics*, **14**, 427) that the joint distribution of the cell frequencies in a 5 x 5 table with fixed marginal totals approaches the multivariate normal distribution in sixteen independent variables, if the total frequency approaches infinity with the row and column totals remaining constant proportions of the grand total. The mean value of the frequency in each cell is the product of corresponding row and column totals divided by the grand total.

If the total frequency is large, the cell frequencies will be distributed approximately in this manner. Hence any sixteen cell frequencies of which not more than four are in the same row or column may be considered as independent, normally distributed variables. This independence means that the mean value of the frequency in a particular "miss" cell would not be appreciably affected by assigning any particular set of values to the diagonal frequencies, so long as the row and column totals are preserved. The "neutral" diagonal frequencies are a convenient mathematical device by which the observed non-diagonal row and column totals are preserved while the cell frequencies may be computed as for a complete 5 x 5 table.

Table 2

ANALYSIS OF D.L. DATA FOR CONSISTENT MISSING
(Three Sessions from Series 2)

CALLS \ CARDS	Blue	Green	Red	White	Yellow	Row Totals
Blue	(176) 139	164 (+)4.54 138.9	139 (−).72 149.1	78 (−).64 85.4	137 (−).40 144.6	518 (657)
Green	125 (−).27 130.9	(166) 131	132 (−).57 141.0	76 (−).26 80.6	156 (+)2.82 136.4	489 (620)
Red	188 (+)9.91 149.5	152 (+).04 149.7	(145) 161	97 (+).26 92.1	110 (−)13.46 155.8	547 (708)
White	56 (−)22.08 103.9	62 (−)16.96 104.0	168 (+)28.13 111.9	(355) 64	142 (+)10.49 108.3	428 (492)
Yellow	156 (+)1.60 141.0	156 (+)1.55 141.2	115 (−)8.96 151.9	94 (+).58 86.9	(155) 147	521 (668)
Column Totals	525 (664)	534 (665)	554 (715)	345 (409)	545 (692)	2503 (3145)

$\Sigma x^2 = 124.24$ (11 d.f.)
$P = 10^{-20}$

In this table the actual number of hits is given in parentheses in the upper right-hand corner of the diagonal cells. These observed hits are for the record only, however, as they do not enter into the consistent missing evaluation in any way. The "expected" number of hits, or the figures for the neutral diagonal, appear in the lower left-hand corner. The three figures in the non-diagonal cells (misses) are as follows: that in the upper right-hand corner shows the observed cases; that in the lower left-hand corner is the expected number of misses computed by means of the neutral diagonal; that in the center is the chi square. The positive sign in front of a chi square shows that the observed figure is larger than the expected value; the negative sign, the reverse. Two sets of row and column totals are shown: the smaller figures are those of the observed misses: the larger figures (in parentheses) are the totals which include the neutral diagonal values and from which the expected

values in the entire table were computed. The total chi square for a 5 x 5 table computed in this way has 11 degrees of freedom, as the fixed values on the diagonal represent five constraints in addition to the nine ordinarily present in a complete 5 x 5 table (d.f. = 25 − 9 − 5 = 11).

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ELECTRIC SHOCKS FOR ERRORS IN ESP CARD TESTS

By W. A. McElroy and Winifred R. K. Brown

ABSTRACT: The investigation described here was designed to test whether learning or improvement with practice takes place in tests of ESP. Thirty-one subjects in an experiment with cards were asked to determine by ESP which of the cards were "red" and which were "black." In half of the trials the subject was given a mild electric shock each time he made an incorrect call.

The results of 37,200 trials show that learning did not take place. They do show, however, that ESP occurred to a greater-than-chance degree, with most of the excess hits occurring in the trials in which the subject was given shocks for failure. The results also show that in these same trials with shocks, there was a statistically significant decline in scoring between the first half and the second half of that portion of the experiment during which shocks were being given.

Miss Brown is a former graduate student in psychology at the University of Glasgow. Mr. McElroy is on the teaching staff of the Department of Psychology.—Ed.

Introduction

It is clear from the accumulated results of the research in extrasensory perception that the use of ESP ability is based upon stimulus cues of minimal strength as compared with the ordinary physical cues upon which sensory perception depends. One can say with certainty in response to visual stimulation that a certain card is the Queen of Clubs, for instance; whereas with extrasensory responses a comparable subjective feeling of certainty is rarely, if ever, present. Thus, even one of the most successful of ESP subjects yet reported in the literature of parapsychology, the talented B.S. studied by Soal, has been unable to state to a better-than-chance degree whether his ESP performance upon any one occasion has been good or bad.

It seems probable that B.S. received a far greater number of extrasensory cues than was indicated by the number of hits which he made. Similarly it follows for any subject calling ESP cards that the amount of extrasensory information which he utilizes must be small in relation to the total amount which he receives, and that his ESP score is thus always limited by his inability to convert extrasensory stimuli into overt responses. Indeed it is entirely

possible that the psi faculty functions at an unconscious level in a continuous manner so that the card-caller receives extrasensory stimuli corresponding to each target card in turn.

On the above assumption, the efficiency of ESP responses reduces essentially to a problem of how well the subject has learned to recognize and act upon his different extrasensory cues. If we attempt to analyze the card-calling situation from this psychological viewpoint, there can be no doubt that the easiest *learning* task for the subject is one consisting of only two choices within the experimental situation. Thus, just as it is easier to teach a child to distinguish between black and red objects than to teach it to distinguish between black, red, orange, purple, and pink objects, so it must be easier, if our assumption is correct, for the subject at ESP card-guessing to distinguish between red and black cards than between cards of five different colors.

Let us suppose that the learning task has been thus simplified for the subject so that only two targets are used in the experimental situation and that these are black and red cards. It is clear that the subject, when he makes his calls, can be aided in distinguishing between the two only by having some knowledge of his results. If he calls "red, red, black, red, etc." and has no information about the success of his calls, then there is no psychological basis for expecting his standard of performance to improve. In order that the primary conditions for learning be provided, therefore, the subject must be told after each guess whether he has hit or missed the target. Furthermore, it is common psychological knowledge that the learning process has an added advantage if the subject is specially motivated in some way to make the "correct" response. In the laboratory this motivation may be provided artificially in the form of an electric shock which is applied each time the subject makes the wrong choice. These two devices: a simple choice between two targets, and the application of electric shocks for errors in identifying the targets are the basis for the experiment reported here. In half of the ESP tests, a shock sufficient to make the subject strive to avoid missing the target was used. This served a double purpose: it was not only a means of motivation but also a constant informer that no extrasensory target had been recognized correctly.

Experimental Plan

The writers had hoped to obtain 36 subjects for the investigation. However, difficulty was encountered in obtaining this number, and the final experimental group consisted of 31 persons. The subjects were chiefly college students and business acquaintances who volunteered for the experiment. Each was accepted after he agreed to make 20 visits to the laboratory to complete two series of 30 card guesses upon each visit. (Five subjects who started the tests dropped out shortly after the beginning. They are not counted among the 31 subjects, and their data were not considered in the evaluation of the results.) Hence each subject made 1200 guesses in all during his 20 visits, and the total number of guesses made during the investigation was $31 \times 1200 = 37{,}200$.

It was considered necessary to divide the experimental group into three sections, each containing an equal number of subjects. Subjects were to be assigned to these sections by drawing names out of a hat. The schedule for each section was as follows:

Section A: Subjects to make 10 successive visits, involving 600 guesses with shocks for errors, followed by 10 successive visits involving a further 600 guesses without shocks for errors.

Section B: Subjects to make 10 successive visits, involving 600 guesses without shocks for errors, followed by 10 successive visits involving a further 600 guesses with shocks for errors.

Section C: Subjects to make 20 visits, upon each visit making two series of 30 guesses. At the first visit, the first series would be made with shocks and the second series without shocks. At the next visit this order would be reversed, and so on, alternately through 20 visits.

This symmetrical arrangement of guesses justifies the experimenters in adding together the total number of hits made with shocks in Sections A, B, and C, and comparing this number with the total number of hits made without shocks in the separate sections and in the experiment as a whole. Since it was impossible to place an equal number of subjects in each section with an experimental group of 31 persons, the odd subject was placed in Section C.

Experimental Situation

The investigation was carried out in W.A.M.'s room in the Psychology Department at Glasgow University between January 1 and March 31, 1950. This room was originally designed as a small child guidance clinic and is conveniently divided into two sections by a permanent wooden screen. Subjects who arrived to take part in the tests while the experimenter was still engaged with a previous subject could therefore enter the room quietly and take a seat in the outer section without interrupting the experimental procedure.

The subject was always seated in a chair at one end of the experimental table with his right side toward the table. His right forearm was strapped to an arm rest on the end of the table in such a manner that the palm of his hand rested on one electrode and his index finger on another. The subject was separated from the experimenter, who sat at the opposite end of the table, by a screen formed of several thicknesses of black cloth which extended across the center of the table and back to the experimenter's end on each side. These screens were arranged so that the subject could not see the experimenter unless he stood up from his chair and so that he could not see the card fixated by the experimenter unless he left his chair and walked around the table to the back of the experimenter.

The setup for the shock may be described in general terms. The source of the electricity was a two-volt storage battery connected in a circuit with a primary induction coil. In the circuit with the subject's electrodes was a secondary induction coil which could be adjusted by the experimenter to vary the strength of the shock. A control switch operated by the experimenter made it possible to shock the subject as required.

The experimenter used a pack of 52 ordinary playing cards. These cards were thoroughly shuffled before each series of guesses made by the subject. For each trial the experimenter drew out a card and fixated it, with a mental set to be aware primarily of its color; that is, as to whether it was a "red" or a "black" card. He then allowed a metronome on the table at his left to make one swing and thus inform the subject that a guess was to be made. It was soon recognized by the experimenters that the use of this metronome might provide the subject with a sensory cue about the target. After

1500 guesses had been made, and at a time when the experimental results showed four hits above chance, the use of this instrument was discontinued. Thereafter the subject was notified just before the experimenter was ready to fixate the first card to be guessed. The subject then went on to call at his own rate, and only upon rare occasions did he have to be interrupted because his rate of calling exceeded the experimenter's rate of working.

If the subject's guess was correct, it was entered immediately on the record sheet. If it was wrong in an experimental session requiring the administration of shock for errors, the experimenter depressed the key to give the shock before making a record of the event. The method of keeping the experimental records was as follows: For each series of thirty guesses a horizontal line was drawn across the sheet. This line was divided into three parts so that each part accommodated ten guesses, as is shown below:

RRB R BRR	R R	RBR RR RBR
RR B	B BBRRBRB	B B

17

"R" was entered for a guess of "red" and "B" for a guess of "black," correct guesses being entered above the line and wrong guesses below.

When the subject's guess had been recorded in the manner described, the experimenter replaced the target card anywhere within the pack, shuffled the pack, and again drew out a card to be fixated. It will be seen from this procedure that for any single guess, the probability of drawing a card of the specified color is always one-half. It cannot therefore be maintained that a subject, in receiving shocks for errors, had at his disposal any information that was of value in naming the color of subsequent cards.

At any one experimental session the subject was required to make two series of 30 guesses. At the end of each series of 30 guesses he was informed of his score. Subjects were usually congratulated upon their performances by the experimenter, or else they were told that they were sure to improve in the next series. Upon several occasions toward the conclusion of the investigation it was necessary to allow some subjects to perform two experimental sessions during the same visit. When this occurred the experimenter

tried as far as possible to allow a liberal interval of time to elapse between the two sessions.

About sixty percent of the guesses were made with W.A.M. as experimenter, and about forty per cent with W.R.K.B. as experimenter. W.A.M. was usually responsible for guesses made up to 5:00 p.m. each day, and upon two nights a week, while W.R.K.B. was responsible upon the other four weekday nights. This cycle was prearranged by the writers according to their availability to attend the laboratory.

Experimental Results

First let us consider the experimental results taken as a whole without considering any sectional differences in test conditions. Table 1 gives the pooled results from Sections A, B, and C in this way, without making any distinction between calls made with or without shocks for errors.

Table 1

Total Results Showing Chronological Distribution in Four Quarters of the Experiment

Group	Total Trials	Deviation 1st Quarter	Deviation 2nd Quarter	Deviation 3rd Quarter	Deviation 4th Quarter	Total Deviation	CR
A.....	12,000	+61	+ 21	−16	+ 2	+ 68	
B.....	12,000	+14	+ 58	+71	− 9	+138	
C.....	13,200	+ 4	+ 55	+ 6	−15	+ 50	
Total...	37,200	+79	+134	+61	−22	+252	2.61

Diff. (1+2) − (3+4) = 174
$CR_d = 1.80$

For the total of 37,200 guesses, 252 were made in excess of chance expectation. The CR of 2.61 shows that the probability of obtaining so large a deviation by chance alone is .01, and it is therefore reasonable to conclude that ESP occurred within the experimental sequence.

Table 1 also shows the sessions for all subjects pooled and divided chronologically into quarters (five visits for each subject for each quarter), again no distinction being made between calls with

or without shocks. A feature of these data is the tendency for the number of hits to fall away toward the conclusion of the investigation, and more particularly within the last quarter; that is, the last five visits. This decline effect, which is in line with previously reported ESP results, is not significant over the data taken as a whole.[1] The CR of the difference between the first and second halves of the experiment is 1.80 (P = .07). The most marked decline is that between the second and fourth quarters where the difference has a CR of 2.28 with P = .02.

The analysis of the results in relation to the use of shocks for failure did not show any evidence of learning in the ordinary sense of improvement with practice but it did reveal two other points of primary interest. The first is the fact that the tests when the shocks were used gave most of the total positive deviation, the total score being 176 hits above mean chance expectation for the punishment runs and only 76 above for the runs without shocks, as shown in Table 2. The runs with shocks, considered alone, gave a CR of 2.58, while those without gave a CR of 1.11. The second important finding is the fact that the runs with shocks account entirely for the chronological decline in scores. Of the trials done with punishment, the first half (10 runs) gave a deviation of 178 hits, while the score on the second half was two hits below mean chance expectation. The difference of 180 hits has a CR of 2.60 (P = .009). The runs without shocks, on the other hand, showed a slight chronological incline, the deviation rising from +11 in the first half to +65 in the second half.

The experimenters classified the subjects in terms of how they reacted to the shock, separating them into two groups according to whether their attitude toward submitting to the shock was "favorable" or "unfavorable," and also dividing them into smaller groups according to the specific type of behavior shown. As none of these

[1] It is important, perhaps, to state that this decline of scoring was not anticipated by the experimenters nor by any of the subjects as far as is known. It was not until a résumé of the experimental results was reported to Dr. R. H. Thouless that the writers became aware that such a decline was a routine feature of ESP performance. The presence of this effect, therefore, tends to confirm the view, stated previously, that ESP occurred within the experimental series. It must be recognized, however, that the planning of the experiment in relation to the problem of learning would probably have been very different if the investigators had anticipated the occurrence of such a decline.

Table 2

TOTAL RESULTS IN RELATION TO USE OF SHOCK

Group	Number Trials	Shock Runs			No-Shock Runs		
		1st Half Dev.	2nd Half Dev.	Total Dev.	1st Half Dev.	2nd Half Dev.	Total Dev.
A.....	12,000	+ 61	+21	+ 82	−16	+ 2	−14
B.....	12,000	+ 71	− 9	+ 62	+14	+58	+72
C.....	13,200	+ 46	−14	+ 32	+13	+ 5	+18
Total...	37,200	+178	− 2	+176	+11	+65	+76
		CR=3.69 (P=.0002)		CR=2.58 (P=.01)			CR=1.11 (P=.27)
			CR_d=2.60 (P=.009)			CR_d=−.74 (P=.46)	

groups gave ESP scores that were significantly different from those of the others, it is not worth while to present the results in detail. After the ESP tests were completed each subject was also given a number of personality tests: Raven's Progressive Matrices, a perceptual test of intelligence; the Bernreuter Personality Inventory scored for neurotic tendency, self-sufficiency, introversion-extroversion, and dominance-submission; a dynamometer test; a perseveration test; and Cattell's Fluency Test.

The score made by each subject on each of these was correlated with his total score on the ESP tests and also with the difference between the ESP scores in tests with shocks and those without shocks. One of the personality tests, the Bernreuter scale for self-sufficiency, was negatively correlated with shock-no shock ESP difference to a degree that exceeded four times the probable error ($-.43 \pm .10$). This correlation thus meets the usual criterion of significance, but it was the only one among sixteen correlations obtained that did so and therefore it can be considered of only suggestive value. In general, thus, it may be said that these efforts to study the individual subjects in terms of differences in their reactions to shock and with respect to certain personality characteristics did not add to the general information about ESP performance obtained from the experiment as a whole.

Conclusions

The principal aim of the investigation was to attempt to "teach" the subjects to distinguish between extrasensory cues by introducing the administration of shocks for errors into the normal card-calling situation. It was hoped that the shocks would provide evidence of a simple learning effect in the performance of the subjects. It is clear from the reported experimental results that no evidence for this simple learning effect was found after the application of the required experimental technique to the 31 non-selected subjects. From this it follows that the task of the card-caller in having to distinguish between extrasensory cues is not psychologically equivalent to tasks of forming simple sensory concepts, these latter tasks providing a clear-cut learning effect when punishment for errors in performance is introduced and maintained. The experiment therefore failed in its primary aim.

The experiment did achieve success in three respects that may contribute toward a better understanding of the ESP process and, even more important, may point the way toward a method of controlling it. First there is the fact that the experiment as a whole gave significant ESP results. The second feature, which might have been predicted on the basis of earlier results, was the fact that the scoring declined toward the expected chance rate as the testing continued. The third aspect of the results is the fact that the ESP success was concentrated in the series where there were shocks for wrong responses; the tests with shocks accounted for the major part of the total positive deviation as well as all of the decline effect. It seems possible that the shocks had an excitement value that had a favorable effect upon the subject's ESP motivation, but that the effect was one that decreased as a function of the amount of experience the subject had with the shocks.

APPENDIX

A Subsidiary Individual Experiment

As a postscript to the investigation reported above, the writers would like to add a brief account of a subsidiary experiment carried out by them on Sunday, September 9, 1950, in the room previously described. This experiment was performed to investigate the effect of administering shocks for errors over a prolonged period of card-calling by one subject. Few subjects from the previous investiga-

tions were willing to sacrifice a Sunday afternoon during the University vacation for the experiment, and it was therefore more convenient for W.A.M. to act as subject for this prolonged series with shocks, W.R.K.B. acting as experimenter. The experimental situation was identical with that described earlier in this report, except that guesses were made in series of 25 instead of 30 because of greater convenience in using printed score sheets which had been received from Duke University. Card-calling commenced at 2:00 p.m. and proceeded continuously except for the time it took for W.R.K.B. to report the score at the end of each run, and for an interval of a few minutes between each block of 10 runs. The original plan was to record 5,000 guesses, but unfortunately a breakdown of the electric circuit prematurely ended the series after 3,250 guesses. The trend of the scoring is given in the following table, which shows the deviation from chance for each block of 250 guesses:

Block	1	2	3	4	5	6	7	8	9	10	11	12	13	Total	CR
Dev...	+3	+22	+11	−8	+12	−6	−2	−2	+15	+8	0	−1	+4	+56	1.96

It will be seen that once more there is no evidence of cumulative learning in the sense of improvement with experience, inasmuch as a decline of 10 hits occurred between the first and second halves of the experiment. There is some indication, however, that W.A.M. recognized correctly a number of extrasensory cues, and hence it is possible that some learning did occur. The question arises, therefore, as to whether he has benefited from this period of possible instruction with respect to future card-calling performances, such that a "transfer of training" might occur from this series to the next. The writers regard this subsidiary experiment as the first of a number of prolonged sessions with shocks which they intend to carry out in the future with W.A.M. as subject in order to examine the possibilities of improvement from one session to another. If any positive evidence of transfer is found, the results of the session of shocks series will be reported in full.

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MINOR ARTICLES

AN EXPLORATORY STUDY OF THE MENTAL HEALTH AND INTELLIGENCE OF ESP SUBJECTS

By Olivia B. Rivers

ABSTRACT: Two groups of subjects (36 college men and 36 high school boys) were given a series of clairvoyance and GESP tests. Each subject was also given a mental health analysis on the basis of which, it was hoped, the subjects could be divided into high and low scorers. While the total ESP scores were statistically significant, the mental health analysis did not prove to be a reliable basis for selecting good ESP subjects in this investigation.

This is the first contribution to the Journal by Mrs. Rivers, a former instructor in psychology, who is now a research assistant in the Parapsychology Laboratory.—Ed.

Research on the problem of finding correlates between ESP and specific personality traits still represents a relatively new area of research in parapsychology. Several investigators have found that good and poor ESP subjects may be distinguished at least to some degree on the basis of their ratings on certain personality tests. Up to the present time only a few such tests have been utilized in ESP investigations and it is possible that others may be found to be even more discriminative in separating ESP scorers. The present paper describes an experiment in which an intelligence test and a hitherto untried personality test were administered to subjects who had taken part in an ESP experiment. The investigation can be regarded only as an exploratory one from the personality-ESP angle, and, in fact, little of suggestive value resulted from the use of either the personality test or the intelligence data. The total ESP results, however, give evidence that ESP was operating.

The Experiment

The experiment was conducted in the city high school at Starkville,[1] Mississippi, from October, 1948, through January, 1949, and

[1] The writer wishes to express her appreciation to Mr. J. W. Overstreet, Superintendent of Schools, Starkville, Mississippi, for his cooperation and interest in making this study possible.

on the campus of Mississippi State College, from February, 1949, through May, 1949. Seventy-two male subjects are represented in the final results of the experiment: 36 were high school students (Series I) and 36 were college students (Series II). The subjects were all given two types of ESP tests: a test of general extrasensory perception (GESP) and a clairvoyance test. A mental health inventory was also administered to all subjects. Intelligence tests were given to all of the high school subjects, and intelligence test scores from the college personnel records were available for slightly over half of the college students.

The ESP tests consisted of four runs of GESP and four runs of clairvoyance, and the standard ESP cards were the basis of all the tests.

The mental health rating which was used for the experiment was the Mental Health Analysis—Secondary Series by Thorpe, Clark, and Tiegs. It is in the form of a questionnaire and was given to most of the high school boys to fill out in a group after the ESP tests were done. In the two or three cases where the ESP tests had not been taken, the experimenter waited to score the Mental Health Analysis until after the ESP test was done. This precaution was adopted to prevent the investigator from forming any opinion about a subject prior to his taking the ESP tests. In the case of the college students, the mental health questionnaire was given to the subject individually to fill out after he had done his ESP runs. Several students were allowed to take the form to their dormitories to fill out and return later.

The Mental Health Analysis is composed in such a way that it is possible to obtain ratings on each subject's "assets" and "liabilities." Factors considered assets are: close personal relationships, interpersonal skills, social participation, satisfying work and recreation, and adequate outlook and goals in life. Factors considered liabilities are: behavioral immaturity, emotional instability, feelings of inadequacy, physical defects, and nervous manifestations. This study was designed to examine the relationships of ESP results to the various percentile scores of the subjects on the specific areas described in the Mental Health Analysis.

The intelligence test employed for the high school subjects was

the California Test of Mental Maturity. It is a "paper and pencil" test that gives scores translatable into a language intelligence quotient and a non-language intelligence quotient.

It was necessary to rely on the college personnel records for intelligence test scores for the college subjects. Scores made on the American Council on Education Psychological Examination for College Freshmen, 1946 edition, were available only for slightly more than half these subjects. This mental test is also made up of two areas of examination, a quantitative, or non-verbal, area and a linguistic, or verbal, area which, considered together, provide a basis for a general intelligence rating. This fact makes it possible to compare the results obtained from this test with those obtained on the California Test of Mental Maturity which, it has been pointed out, is organized on a similar basis.

The Procedure

Series I

Of the 36 high school subjects, all except two were juniors; the remaining two were seniors. Each of the boys was called individually to a small room on the first floor of the high school building to take the ESP tests. Upon entering the room, he was told briefly what to do and what the tests were designed to discover. No detailed explanation of ESP was attempted; it was the hope of the experimenter that she said just enough to each boy to arouse his interest and to motivate him to try to perform well on the tests.

The ESP test procedure was the same for all subjects. The first four runs were a clairvoyance matching technique. In this part of the test the experimenter placed five ESP target cards on the table in front of the subject and gave him a deck of 25 ESP cards which had been shuffled and placed in opaque black envelopes by the experimenter before the experiment started. The subject was asked to try to "see" the card inside each individual envelope and to distribute the cards in the deck one by one in front of the target cards which he thought they matched.

At the end of each run the experimenter opened the envelopes and recorded the symbols on an ESP record sheet. She and the subject checked the hits and recorded the correct number at the bottom of the record blank.

When the fourth clairvoyance run had been completed, the subject was told that he was going to be given four GESP runs, and he was encouraged to believe he would find these much easier than the ones he had just done. The experimenter almost always was able to arouse renewed interest on the part of the subject before beginning the second test. This attitude was thought to help overcome decline effects which might have appeared during the four runs of the clairvoyance test.

In this second part of the test session the subject was seated at a distance of about 12 feet diagonally across from the experimenter, partly facing her. The technique used was a simple GESP procedure in which the experimenter turned over the top card of a deck of previously prepared ESP cards and looked at it while the subject tried to call the correct symbol. A screen 15 inches high on the table in front of the experimenter concealed her and the cards from the subject's view. As each card was called, she recorded the subject's response in the "call" column of an ESP record sheet, laid aside the card, and turned over the next card on the pile.

At the end of the run the experimenter turned over her deck of target cards and recorded them in the original order in the "card" column, adjacent to the subject's calls. The subject then watched her go down the two columns, check the hits, and record the score at the bottom of the sheet.

The subject knew his score at the conclusion of each run, and the experimenter praised him throughout the entire experiment whenever possible. At the end of the test session the subject was thanked and given some small reward (a candy bar, soft drink, or movie ticket) in proportion to the amount of success in scoring he had shown on the test.

The experimenter rechecked all record sheets at a later date. A third checking was made still later to avoid the possibility of errors in tabulation.

Series II

All of the 36 male subjects in Series II were second semester college sophomores or higher. The list of prospective subjects for this series was drawn up by a committee of faculty members and students. It was felt that the college group would be more highly se-

lected than the high school group unless specific care were exerted to make it representative. Therefore a special attempt was made to secure subjects for Series II who would show wide variation in general ability and socio-economic background. Those who finally made up the group came from delta plantation homes, small farms, large urban centers, and small-town middle-class sections. The high school group included boys from the exclusive residential sections of the town, the middle-class districts, and the mill areas. The experimenter was unacquainted with the personal data regarding either group of subjects until they had taken their ESP tests.

The subjects for Series II were called together in a general meeting at which the experimenter explained something about the experiment to be conducted. At the end of the meeting each boy was given an opportunity to sign up for the experiment. Most of the boys showed a desire to take part in the study, but as time passed it became necessary to substitute a few new subjects for those who found they could not complete the tests.

The ESP tests for Series II were conducted in one of two different rooms on the college campus. The testing conditions were the same as those used for the subjects in Series I and with one exception the same procedures were followed. In the second series, alternate subjects took the four runs of GESP first and the four runs of clairvoyance next. The other half of the subjects reversed the procedures. All subjects in Series I took the four runs of clairvoyance first. In no other respect did the administration of the ESP tests in the two series differ.

Because of their higher age level, the subjects in Series II were not given any rewards other than light refreshments. Since the rewards in Series I were not given until after the subjects had finished their tests and since the subjects did not know that they were to be given specific rewards, it is not likely that they were favored by any motivational device which was lacking in Series II.

Results

ESP Tests

The students in the high school group made 288 ESP runs in all, with a total deviation of +105. The CR for this group alone is 3.09, which is statistically significant. There was no marked dif-

ference in scoring level between the GESP and clairvoyance tests. As Table 1 shows, the 144 runs of clairvoyance gave a deviation of +58, while the same number of runs for GESP had a deviation of +47.

Table 1
GENERAL RESULTS

Group	CLAIRVOYANCE			GESP			TOTAL		
	Runs	Dev.	CR	Runs	Dev.	CR	Runs	Dev.	CR
High School.....	144	+58	2.42	144	+47	1.96	288	+105	3.09
College.....	144	+ 8	.33	144	+33	1.38	288	+ 41	1.21
Total.....	288	+66	1.94	288	+80	2.36	576	+146	3.04
									P = .002

The college group also completed 288 ESP runs, but their total deviation was only +41, which is not significantly different from chance. The subjects in this group made 144 runs for clairvoyance with a deviation of +8, and 144 runs for GESP with a deviation of +33. None of the results for the college group alone proved statistically significant.

When both groups are pooled, the GESP tests may be considered marginally significant (CR = 2.36). The clairvoyance tests, while positive, have a higher probability of approximately .05. Altogether 576 ESP runs were made by the two groups of subjects. The deviation is +146 and the CR, 3.04. The pooled total, then, is statistically significant, with a probability of .002.

Comparison of the average scores for the four runs of clairvoyance and the four runs of GESP showed no marked difference in position effects for either group of subjects.

The total clairvoyance score for each subject was correlated with his total GESP results. For Series I, the high school group, the coefficient of correlation was +.30, which, for 36 cases, is not statistically significant. The correlation for the 36 cases in Series II, the college group, yielded a coefficient of −.25, which also does not reach the .01 criterion for significance. It appears, therefore, that the subjects' scores in the one type of test were not related to those in the other.

Mental Health Test Data and ESP Results

Inasmuch as the present study was an exploratory one involving the first use of the Mental Health Analysis in conjunction with ESP tests, no conclusions can be reached as to the relationships between ESP scores and the various mental health ratings. From the first use of a personality test one may by inspection or by tests of significance arrive at certain hypotheses as to the relation between the ESP scores and certain personality ratings. It must remain the task of future research to test the hypotheses so gained.

In analyzing the present data for clues as to possible relations between ESP and personality ratings, the investigator converted the raw scores on the personality tests into appropriate percentile scores indicating the subjects' standing in high school and college populations. The experimenter grouped these ratings into intervals of ten percentiles, and opposite each percentile she placed the clairvoyance and GESP results of each subject whose personality score fell within that interval. The distribution of scores was then divided in two so as to give the best possible separation of high-scoring and low-scoring ESP subjects. The method of analysis is illustrated in the table below where the clairvoyance test deviation for each subject in the high school group is listed opposite the percentile score which the subject obtained on the rating called "Freedom from Nervous Mannerisms."

From an inspection of the distribution of ESP scores, it may be seen that the scores of subjects at the sixtieth percentile or higher were predominantly negative, while those below the sixtieth percentile were positive. If the distribution of ESP scores is divided between the fifty-ninth and sixtieth percentiles, we have two groups of subjects whose ESP scores are significantly different, with the $CR_d = 3.12$. Of course we can attach no significance to a CR based on data separated in this manner, but it suggests that the use of this particular personality rating in this way may separate high and low scorers in other ESP test data. Accordingly, the GESP data for these same high school subjects were divided into two groups on the basis of the subjects' ratings on "freedom from nervous mannerisms." Those subjects at the sixtieth percentile and above gave negative GESP scores (48 runs, −17), while those below the sixtieth percentile gave a total GESP score above mean chance expec-

Table 2

CLAIRVOYANCE SCORES FOR HIGH SCHOOL SUBJECTS IN RELATION TO PERCENTILE RATINGS ON "FREEDOM FROM NERVOUS MANNERISMS"

Percentile	Individual Subjects' Clairvoyance Scores (4 runs per subject)	TOTAL		
		Runs	Dev.	
90-100	−2	4	− 2	
80-89	+3,−4,−6,−4	16	−11	48 runs
70-79	+5,+1,−3	12	+ 3	−16 dev.
60-69	−1,+2,−4,−3	16	− 6	
50-59	0,+7,+5,+6,+17	20	+35	
40-49	+11,−1,+3,−2,−5,+5,+3	28	+14	
30-39	+9,+8,−1,−2	16	+14	96 runs
20-29	+6,+2,−1,−5	16	+ 2	+74 dev.
10-19	−1,+10,0	12	+ 9	
0-9	0	4	0	

tation (96 runs, +64). The CR of the difference is 2.89.

When the same analysis was applied to the data of Series II, however, to determine whether the college group followed the same trend, it was found that there was no marked separation of ESP scores by the "nervous mannerisms" rating.

The ESP results and the subjects' percentile scores on the other mental health ratings were similarly analyzed, and it appeared that in the high school group the subjects who showed relatively marked "behavioral immaturity," "feelings of inadequacy," and "emotional instability" gave higher scores on the clairvoyance tests than did those who were relatively "free" of those characteristics. These same "liabilities" ratings gave only negligible differences in the separation of the GESP scores of the high school group, however, and none of the mental health ratings showed any relation to the ESP scoring level of the college subjects.[2]

Intelligence Test Data and ESP Results

The intelligence test data showed no relationship to ESP scoring for either group of subjects. There seemed to be nothing of sufficient interest in the total intelligence scores or in either the quantitative or the linguistic areas of the tests to make it feasible to review these data in the present paper.

[2] These figures may be obtained from the author by anyone who wishes to have them.

Discussion

The subjects in Series I gave higher scores than those in Series II—sufficiently higher to warrant some consideration of the subjects involved in each group with a view to picking out any differentiating characteristics between them.

Although such comparisons must of necessity involve subjective decisions on the part of the investigator, the writer feels that with regard to general ability and socio-economic background the two groups were relatively comparable. It was assumed that the high school class would provide a more or less natural distribution of these two qualities among its members but that a college group would be more highly selected unless specific care were exerted to make it representative. Therefore a special attempt was made to secure subjects for the college series who would show similar variation.

If we look for psychological differences, it would seem that the subjects in Series II were working in a more favorable situation for making high ESP scores. Presumably they took part in the test only because they were interested. Many had attended group discussions of ESP led by the experimenter or had talked privately with her about ESP before they came to be tested. Some were interested enough to begin reading on the subject. The high school subjects, on the other hand, although not compelled to perform, were more or less obliged to comply with the principal's request that they do so. It was obvious that several were antagonistic, and others took part with apparent reluctance and distaste.

One would suppose, then, that the subjects in Series I were at a disadvantage psychologically and would have had lower scores than the other group; yet the results indicate the opposite. Whatever characteristics seem to distinguish the two groups must be sought apparently in other directions, possibly in differences in maturational level and emotional maturity. Certainly the subjects in Series I were younger and more immature than those in the other group. On the average, the subjects in Series I were about 17 years old, whereas those in Series II ranged from 22 to 25. Many of the latter were veterans, a fact which raised their age above the expected level for college students. These factors in themselves are worthy

of further study. Whatever relationship they hold to high-scoring ability on ESP tests is a challenging research problem.

Perhaps the high-scoring younger group of subjects were the more suggestible and accepted the favorable approach to the whole subject presented by the investigator more easily than did the less suggestible, more mature, older college group of subjects. The writer was trying to exert the most favorable influence possible on the subjects and tried to get each individual subject to perform with confidence in the idea that he, personally, possessed ESP ability. Most of the college subjects seemed convinced that psi factors exist, but many of them expressed doubt that they personally possessed any ESP ability.

It seems reasonable to think that the experimenter's positive manner and highly expectant attitude regarding high scores from the individual subject had more effect on the younger, more insecure, more responsive subjects. High school students are probably more accustomed to accepting suggestive approaches made by their instructors than are college students, who, presumably, would be more inclined to disregard the instructor's comments if these did not fit in with their preconceived ideas of the topic under discussion. This of course is only a hypothesis which would be an interesting topic for further research.

Mere speculation leads the investigator to suggest that the greater knowledge of ESP on the part of the college group may actually have hindered their scoring ability, since possibly they let logic and reasoning persuade them to distribute their calls among the five symbols in a manner that was somewhat orderly or unconsciously planned, rather than let spontaneity or impulse govern their response to the target cards. Perhaps complete lack of inhibition regarding the subject of ESP on the part of the high school group might have been to the advantage of the individual subjects.

This study suggests also that the attitude of the examiner as it influences the subject is worthy of further investigation. The writer feels that she did not exert herself with the college group to the extent that she did with the high school group in trying to get high scores from the individual subject for fear that undue enthusiasm on her part might make her appear unscientific in her approach. Perhaps

this greater pitch of tension and enthusiasm, which she did employ with Series I, is necessary for favorable response from the subject.

The ratings on the mental health inventory suggest that the subjects in Series I who were the most immature, the most unstable emotionally, and who gave the most evidence of feelings of inadequacy and nervous mannerisms were the ones who obtained the highest ESP scores on the clairvoyance tests. These findings were not confirmed by the results of the college students in Series II, however; and the inventory cannot be considered as promising a method of selecting ESP subjects as others which have previously been reported.

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SOME EXPLORATORY TESTS IN DOWSING

By J. B. Rhine

ABSTRACT: In this short experiment two persons were tested for dowsing ability. They walked back and forth over an underground water pipe while holding a "divining rod" (in this case a forked twig) in a horizontal position. If the twig turned downward it was taken to indicate that the water was flowing; if not, the water was supposedly turned off.

Seventy trials were done with a procedure in which the flow of water was determined separately for each trial. In this part of the experiment the subjects avoided the correct choice to an extrachance degree. Sixty trials were done with a method in which the flow of water was on or off for five consecutive trials at a time. In this section the subjects scored somewhat above the average expected from chance. The difference between the two conditions was found to be statistically significant. A marked decline in scoring rate was found in the ten-trial run for the experiment as a whole.—Ed.

On November 5 and 6, 1949, I visited the well-known novelist, Kenneth Roberts, at Kennebunk Port, Maine, for the purpose of conducting some tests with Henry Gross, a neighbor of his, who is reputed to have remarkable dowsing ability. While there, I had the opportunity incidentally of testing another neighbor of Mr. Roberts, Mrs. A.V.M. Henry Gross is the subject of Kenneth Roberts' latest book, *Henry Gross and the Divining Rod,* in which his many exploits with the forked twig are reported. He is a normal, healthy, friendly man of middle age who has been until recently a state game warden. Mrs. A.V.M., a young woman, is a novelist and the daughter of a well-known scientist. Her dowsing experience has not been extensive and her participation in the tests came about quite incidentally; Henry Gross was the primary object of the testing.

The tests were all carried out with the co-operation of Mr. Roberts and Dr. Horace Levinson. Along with his other scientific qualifications, Dr. Levinson is an authority on the mathematics of probability (and is the author of a popular book on probability, *Your Chance to Win*).

The experiment, which lasted through one afternoon and the forenoon of the next day, was prefaced by several hours of discus-

sion and demonstration of the way in which Henry Gross is reputed to locate underground water and tell its depth, quality, and direction and rate of flow. I ascertained that Mr. Gross felt confident his dowsing rod would also work in detecting whether or not the water was flowing in an underground pipe. Since he was convinced that it would, the underground pipe was adopted as the basis of the test procedure.

Two sites were used for the tests: one, the underground pipe leading from a spring to Mr. Roberts' barn, and the other, the underground pipe leading from the spring to his house. The tests on the barn-line were conducted with Gross approximately 150 feet from the barn and were made with the doors and windows open to allow for communication. Those on the house-line were made at a distance of fifty feet or a little more, with the doors and windows of the house closed.

The test procedure was as follows: the dowser was instructed to walk over the ground under which the pipe lay, holding his twig in position. If the rod turned downward it was supposed to indicate that water was flowing; if the rod did not move it was taken to indicate that water was not flowing. Dr. Levinson remained outside with H.G. and recorded the indication of the rod. He did not know whether the water was on or off. Mr. Roberts was in charge of the turning-on and -off of the faucet and could not see, or be seen by, H.G. during the tests. Arrangements were made that no other faucet in the building concerned was to be used during the tests.

I was stationed indoors with Mr. Roberts and at the beginning of each trial I indicated to him whether or not the water was to be on or off. This decision was based on the toss of a coin. After twenty trials or two runs, however, the coin was used only to determine the first trial out of each five and a pattern of on-off sequences for the next four trials was designed mentally (and recorded in advance). This was done to keep the number of on and off trials equalized for each run of ten trials. The pattern was varied from run to run. I called "ready" to Dr. Levinson for each trial at the barn, and at the house signalled with a hand at the window. The dowser had no opportunity to learn anything from these signals.

He did not learn after each trial whether or not he was right. We checked up after each run but the dowser was not told the score. He was told the results were not all hits but were "very interesting." (He did know from Mr. Roberts' manner, however, that the scores were not pleasing him.)

Results

In all, 13 runs (or 130 responses) were recorded in the experiment to determine whether or not the flow of water in the pipe could be detected. Fifty of the 130 were done on the house-line and 80 on the barn-line. Since there was no important difference in any respect between the two groups of trials, I need not separate the results on this basis in reporting the figures that follow.

The tests do, however, fall naturally into two groups which I will call Sections I and II. In Section I will be the tests in which the target (on or off) was determined separately for each trial; these all came first in time with the exception of one run inserted in the midst of the later section as a control. There were 7 runs, or 70 trials, of this type; 60 trials by H.G. and 10 by A.V.M. Section II consists of 6 runs carried out to test a suggestion by Dr. Levinson that longer continuance of the same target condition might raise the scoring rate. Accordingly the water was kept either on or off for five trials in succession. (The subject did not at any time during the experiment know of this change in the way of varying the target.) The selection as to which came first (on or off) in the run was made by the toss of a coin. Five runs (50 trials) were made by H.G. and one (ten trials) by A.V.M.

Altogether the 7 runs of Section I gave only 21 hits, instead of the 35 successes expected on the average from chance alone; that is, there were 21 times when the dowsing rod gave the right indication as to whether or not the water was running. This meant that the dowser was wrong 14 times beyond the number most expected from pure chance. Such a deviation on either side of the chance mean would not be expected in a chance series as often as once in a thousand such series;[1] so it may be considered that something was

[1] The fact that in 40 of the 70 trials the targets were chosen mentally while the other 30 were selected by coin throws technically could introduce a slight error in evaluation. However, the average scores were the same for the first two runs, in which each target was selected by coin throws, as they were for the other five. Also, the CR of the deviation (3.35) is large enough to stand a great deal of correction.

taking place besides chance events. (A cross-check of the targets of one run against the calls of the next run gives 37 hits instead of the expectation of 35.)

What is more, both dowsers contributed to this curious negative effect. H.G. scored 10 points below expectation on his 6 runs and A.V.M.'s single run gave 4 points below. The consistency of the trend is itself impressive: each of the 7 runs gave a score below the number 5 which would be the chance average. This degree of consistency in scoring would be expected only once in 64 such series.

Dr. Levinson's suggestion that we use the same target for 5 successive trials worked as he had expected: it did raise the scoring rate. In Section II there were 50 trials by H.G. and 10 by A.V.M. with the target condition (water turned off or on) continuous for each set of 5 trials. These 6 runs gave a total of 38 hits, or 8 above the number most likely to occur by chance. This subseries is not in itself significant, but the difference (of 22 hits) between it and the preceding series[2] is quite significant (CR = 3.79). (The cross-check of these runs gave 27 hits where 30 were expected.)

Again there is a consistency that is impressive. First, both subjects contributed to the positive deviation, H.G.'s 5 runs giving 5 hits above expectation, and A.V.M.'s one run, 3 hits above. Second, of the 6 run scores, not one is below 5. Thus there was produced in the experiment as a whole a highly consistent set of score data, even though the brevity of my visit limited the number of trials and the informality of the tests left some points for later work. The internal consistency, however, goes far toward making up for the limitation in the number of trials. At any rate, both the marked significance and consistency of the results as well as the control over the conditions are sufficient to justify further investigation of the problem. No more need be claimed for such an experiment at this stage of the research on dowsing.

There is a further item concerning the results that merits attention. It deals with a hit distribution or position effect which has

[2] It is only by an approximate estimate of significance that we can compare the results of these two sections in which the targets, especially those of Section II, were selected by a nonrandom procedure. However, the CR of the difference is so large as to allow a wide margin for error; and since the experiment represents an introductory inquiry into dowsing under field conditions due allowance can reasonably be made.

come to be fairly typical of psi phenomena in general. As is now well known, one of the most consistent characteristics of performance in psi tests is the decline of success in the run or record column. There is even a significant precedent for this characteristic in the very limited literature of experiments with the divining rod. In Miss McMahan's article (1) reviewing literature on dowsing for this JOURNAL in September, 1947, there was included at the end a brief description of a series of exploratory tests I carried out with Mr. A., a business executive who believed he had dowsing capacity. Four runs of 10 trials each were made in a test essentially similar to that described for Section I of this paper; that is, with a randomly selected target for each trial. The total of 40 trials gave only 24 hits, or 4 above the number most likely to be given by a pure chance series, but it was noteworthy that of the total of 24 hits, 16 occurred in the first half of the run when the 4 runs are pooled whereas 8 occurred in the second half. This difference of 8 hits between the first and second halves of the run was statistically significant, although it is based on too small a series to be considered conclusive.

In the present work also it is interesting to see that the same feature of more hits in the first half of the run occurs in both sections of the data; that is, not only with the randomized target but also with the continuous target. The 21 hits of Section I were distributed with 13 in the first half and 8 in the second. Of the 38 hits of Section II 22 hits were in the first half and 16 in the second. This makes, for the total, 35 in the first half and 24 in the second. The difference of 11 hits is not quite significant statistically. It would have taken a difference of 15 to meet that requirement. Here again both subjects gave declines. The decline from the top to the bottom half of the run for the 130 trials of this report and the 40 trials with Mr. A. combined shows a significant difference (CR = 2.91) between the two halves. This result is enough to justify further tests and the use of this type of procedure and analysis. The figures are given below.

In the cross-check of responses against targets not intended the two sections together gave 29 hits in the upper half of the run and 35 for the lower half. In fact none of the characteristics of the results of the experiment was reflected in the cross-check.

	Mr. A. Series		Present Report				Pooled Series	
			Section I		Section II			
	No. of Trials	Hits	No. of Trials	Hits	No. of Trials	Hits	No. of Trials	Hits
1st half-run.....	20	16	35	13	30	22	85	51
2nd half-run.....	20	8	35	8	30	16	85	32

$CR_d = 2.91$

Discussion

From the point of view of interpretation of the results it is fortunate that the deviation of the 70 randomized trials was negative instead of positive. Moderately good precautions (the best possible under the circumstances) were taken to insure that the dowser could not sense any vibrations in the air or ground that would give him clues as to whether or not the water was flowing in the pipe. However, it was not possible to guarantee with finality that the dowser did not possess some special auditory endowment, a hyperacuity of the ear or skin or other receptors. As it is, it would be a great deal harder to suppose that the two subjects, trying as they consciously were to produce a hundred per cent perfect score, were utilizing sensory cues from the pipe to help them in making a large number of errors. The investigators concurred in dismissing the hypothesis of auditory cues.

Obviously it requires as much of a connection between subject and object (call it knowledge or mere responsiveness) to do significant missing as it does to do significant hitting of the target in such a case; that is, to be consistently wrong or consistently right. Mr. Roberts, bent on practical utilization of the dowser's art in the interest of man's growing need for water, was not pleased with these negative deviations in spite of their statistical significance; but from the scientific point of view the directon of deviation made no difference in its significance, and the direction taken was actually the safer one.

Psychologically these remarkably negative results are comprehensible and are not without precedent in the investigation of psi capacities in other situations. There had been a long anticipation

and discussion of this investigation, and the dowsers may well have felt put on the spot. They may have experienced a certain amount of anxiety over the outcome, whether or not they were very conscious of it. At any rate, in the exercise of so delicate a capacity as must be involved in dowsing, delicate shades of attitude that are ordinarily unimportant may be very decisive.

It is not unreasonable to consider unconscious negativism as a possible motivating factor, a desire on the part of the dowser to avoid conforming to the experimenter's plan and procedure. The dowser might conceivably have had some quite unconscious resentment at being forced into so artificial a practice of his art. He may have thought it a foolish type of test. On her part A.V.M. had no reputation at stake and her participation was motivated by her own interest. Still, she had a healthy ego-involvement, and may have feared failure in this "scientific" test. It appears most reasonable, however, to suppose that something in the delicate operation represented in dowsing was sufficiently interfered with to make it function in a wrongly selective way. That is, by doing some crucial step in some way that was different from the usual, the right response was avoided, and the wrong one made. Even though we are still far from understanding the actual mechanism involved, this much of an explanatory hypothesis can be considered plausible. (It is the problem connected with the "sign factor" described in recent psi reports.)

But it is still more difficult to explain the change-over that followed when the water was left on or off continuously for a five-trial stretch at a time. The subjects were not aware of the change, and yet there was a consistent and significant swing in the results to the positive side of the mean. Even so, there was in these positive scores less evidence of "knowledge" (or whatever it is that unconsciously guides the dowser in his automatism) than when the scores were low. The 70 randomized target trials with their negative deviation of 14 (a negative deviation of 2.00 per run) actually gave more evidence of psi capacity than did the 60 trials with their positive deviation of 8 (a deviation of 1.37 hits per run). Also, both dowsers went farther to the negative side in Section I than they did to the positive side in Section II.

Was it that the mere continuance of the stillness or flow of the water provided a better target? Had that been the case, we should expect the rate of success to increase with the order of trials in the set of five, and the fourth and fifth would be the highest in scoring rate. Actually the pooled scores for the successes in the set of five trials are as follows: 8, 9, 6, 8, 7. Accordingly it looks as if it was not the physical condition of the stimulus that made the difference.

Rather, we must look as suggested to the psychological conditions, or perhaps better expressed, parapsychological conditions, that prevailed. Possibly in his function of operating the faucet Mr. Roberts may have been a determining factor. He favored Dr. Levinson's idea of continuous flow, and he was profoundly disturbed by what he regarded as the failure of the other method (in Section I). He may unconsciously have been playing the role of telepathic sender, and if so he may have greatly altered his attitude during the second part of the experiment. It is merely a point to be considered in any further inquiry along this line.

The decline effect, which has been found to be such a consistent characteristic of psi results, turns up even in this radical departure from the formal laboratory test. From the fact that both Section I and Section II showed declines it is quite evident that we are dealing here with a different factor from the sign factor, which determines whether the scores are to be above mean chance expectation or below. The decline factor seems to affect the strength or effectiveness of the ESP process itself rather than the tendency of it to hit or miss the target in the consistent way that the sign factor appears to operate. The basis for the suggestion here is not a very extensive one, but the suggestion itself is important and can be followed through into other analyses of more comprehensive character.

Where does the dowsing phenomenon belong with respect to parapsychology? No way has been devised as yet to exclude either telepathy or clairvoyance from the dowsing test. It would be difficult to do so. But on the other hand there is no reason to consider any of the evidence of dowsing on record as not wholly within the scope of ESP. The unconscious muscular action represented in the mode of response is too common and familiar to be puzzling any longer in itself, and I know of no good evidence for any of the physi-

cal theories held by some dowsers (though not by all). That ESP does express itself through automatisms has already been indicated, though the evidence is not of the best.

Probably the primary problem posed for psi research by the dowser is whether the use of automatisms is a more effective way of demonstrating psi than are the more conscious modes of response. So far we have only the field reports to sustain the view that it is, and these, naturally, are not adequately controlled observations. They do, however, justify attention and only the extension of methods of research to the field situation can give a fair answer.

It should not be forgotten that there may be a wide gap between the free informal practice of dowsing in the field and the performance in a scientific test with investigators officiating. Both Mr. Roberts and Dr. Levinson definitely think there is such a gap. Psi abilities in general are known to be easily inhibited, and there is no sufficient reason to think that the use of an automatic response entirely exempts a subject from these inhibitions. Even the transition of the dowser to the simple condition of having his calls checked for accuracy as soon as he has made them may affect his very elusive powers. Certainly it confronts him with a new and different situation. Until we know all about the ability in question we must make every allowance that could be important. We must do so when possible not only in the testing but also in the drawing of conclusions. Only thus can we be sure we will not miss some valuable clue to the control of the capacities we are studying.

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BOOK REVIEWS

ERGEBNISSE OKKULTER FORSCHUNG (Results of Occult Research), by Rudolf Tischner. Stuttgart: Deutsche Verlagsanstalt, 1950.

This book by the German Nestor of parapsychology is based on his *Einführung in den Okkultismus und Spiritismus* (Introduction into Occultism and Spiritualism), published in 1921. It gives an extensive and critical survey of our present knowledge of parapsychological phenomena. The rich experience of the author in experimentation, his thorough knowledge of the history of the field, and his solid background in theory and philosophy are combined in a rare measure so that this book, written with a commanding mastery of the material, is a stimulating one for the parapsychologist.

Another advantage of the book is that Tischner, being a medical doctor, takes into consideration to a great extent the discoveries in the domain of the subconscious in the explanation of these phenomena.

In the book the experiments of Rhine are given their merited place. Tischner calls Rhine "the Lavoisier of parapsychology" because the latter has done essentially the same for parapsychology as Lavoisier, by introducing the scale balance and thereby quantitative measurement, did for chemistry. According to Tischner, Rhine's term, "extrasensory perception," does not quite hit the essence of the thing, since very often no experiences of a perceptual kind are concerned. Instead it would be better, Tischner thinks, to say "extrasensory experience," as he used the term in his treatise of 1921.

In an article published in 1925 Tischner pointed out that clairvoyance might account for the results of telepathy experiments using token objects, although he did not at that time carry out this idea methodologically. It is, he says, the merit of Rhine to have realized the full importance of this idea and to have carried it out from a methodological point of view.

According to Tischner, so-called "mind-reading" (which he also

calls "telepathic clairvoyance") holds a kind of middle position between telepathy and clairvoyance. Rhine's DT experiments in clairvoyance, during which the deck of cards remains in its place without being touched, are called "boring" experiments by Tischner because the subject "bores" through the deck from the top to the bottom. He has made use of this method in his own experiments; and in several hundred trials with each of several subjects, he obtained an average of from about eight to ten hits for twenty-five calls. One subject had an average of 7.9 hits in 1275 trials.

Tischner cannot agree with Rhine's opinion that telepathy on the whole is rendered dubious by the evidence for precognitive clairvoyance (that is, by the precognition of subsequently made record sheets). There was only a *small* positive deviation in the precognition experiments cited by Rhine, and this might be accounted for by a small error in evaluation, Tischner believes. Moreover, this precognitive clairvoyance hypothesis would not apply to certain spontaneous cases which Tischner thinks necessitate a telepathic contact.

Tischner also deals with the PK experiments *in extenso*. He thinks they need further confirmation, since certain amazing facts are as yet unexplained; for example, the dice experiments in which PK was more effective from a distance than near-by. He suggests that instead of testing PK with dice we use a highly sensitive self-registering apparatus.

In connection with the psychic memory theory of the late Munich philosopher, E. Becher, Tischner wrote a theoretical discussion in 1918 entitled "Über die Wellentheorie der Gedankenübertragung und das Hellsehen" in which he dealt with the idea that these parapsychological phenomena might not necessarily be connected with processes of the brain and that there is the possibility of an extrabodily action of the mind. His remarks prove a valuable beginning, at least a "prototheses" in the direction in which Driesch (giving the thought a more metaphysical trend), Thouless and Wiesner (in a more empirical physiological-psychological way), and also Rhine have worked. Since Rhine has become the leader in parapsychology, using laboratory methods in connection with the opportunities afforded by a university, Tischner is confident about the future of the

field. His book closes with the words: "A whole continent emerging from the ocean of ignorance is awaiting exploration."

ALFRED WINTERSTEIN.

(Dr. Winterstein, a Viennese psychiatrist, has for many years been president of the Austrian Society for Psychical Research.)

TELEPATHIE UND HELLSEHEN *(Telepathy and Clairvoyance)* by Alfred Winterstein. Second Edition. Vienna, 1948.

The author, a Viennese psychiatrist, has for years been the president of the Austrian Society for Psychical Research. He undertakes in the present work to review the entire body of scientific knowledge of parapsychological phenomena. Such compendium-like summaries may indirectly contribute to the progress of science by providing critical examination, by synthesizing the different theories, and also by pointing out aspects of the field where progress is most needed to restore a proper balance.

Winterstein shows himself in the book to be an extraordinary expert on the extensive literature. To establish order and system therein involves difficulties which should not be underrated. Nevertheless it has to be said that the author has only partly succeeded in his systematic coordination of the recent work. A critical examination of the extensive material in parapsychology should have led to a comparison of the values of the different explorations. Such a comparison would have brought to the fore the most modern research as well as any existing criticisms of the work. But this critical examination of the value of the research results is missing in Winterstein's book. Although these modern explorations should be the backbone of such a book, this does not imply any undervaluation of, or even disregard of, the older authors and researches. It would be a rather sorry state of affairs if there were no improvement in the methods and qualities of research work.

Moreover, pointing out the most outstanding research results would have opened the way to discussion and criticism of them and would have revealed any possible onesidedness. It would have been in order, for instance, to show the methodological onesidedness of

the American parapsychological researches despite their significant, even decisive, importance. While in Europe phenomenology (introspection) wins a larger and larger place in psychology, Americans (probably out of a behavioristic prejudice) still regard a phenomenological description of a subjective experience as unscientific, and they restrict themselves to the objective consequences of psychic activities. Thus Americans show themselves still to be completely dependent on the methodology of behaviorism despite a complete change of problem emphasis. This may be a tactical advantage in the fight against materialistic psychology in America. Winterstein had the opportunity here, however, to offer some clarifying criticism and to suggest a synthesis. He made very little use of the opportunity.

Perhaps we are asking too much. After all, it is already something if a book directs the attention of the educated public to an area of science whose importance today cannot even be estimated. Therefore this book, despite its shortcomings, is a contribution in the discussion of and the fight for the human individual.

WILFRIED DAIM.

(Dr. Daim is a consulting psychiatrist in Vienna.)

ERRATA

The following errors appeared in the article entitled "Ehrenfels' 'Explanation of Prophecy' " by Dr. Mildred Focht in the September number of the JOURNAL.

P. 209, line 4: "put" should be "puts"
P. 210, line 17: "insect-stages" should be "insect-states"
P. 210, line 26: "the body of national soul" should be "the body of a national soul"
P. 211, line 6: "center" should be "central"

GLOSSARY

In order to avoid constant redefining of commonly recurring terms in papers appearing in this JOURNAL, the following definitions are submitted for convenient reference.

A simple description of the basic experimental methods and of the statistical procedures to be followed in evaluating ESP (both card tests and drawings tests) and PK may be found in the *Handbook of Tests in Parapsychology* by Betty M. Humphrey (published by the Parapsychology Laboratory, Duke University, Durham, N. C.).

AGENT: In tests for telepathy, the person whose mental states are to be apprehended by the percipient. In GESP tests, the person who looks at the stimulus object.

AVERAGE SCORE: Average number of hits per run.

BT ("Broken Technique"): The clairvoyance technique in which each card is laid aside by the experimenter as it is called by the subject. The check-up is made at the end of the run.

CALL: The ESP symbol selected by the subject in trying to identify a target.

CHANCE: The complex of undefined causal factors irrelevant to the purpose at hand.

CHANCE EXPECTATION = MEAN CHANCE EXPECTATION: The most likely score if only chance obtains.

CHANCE AVERAGE: Mean chance expectation expressed as an expected score, generally in terms of average per run.

CHI SQUARE: A sum of quantities each of which is a deviation squared divided by an expected value. Also a sum of the squares of CR's.

CLAIRVOYANCE: Extrasensory perception of objective events as distinguished from telepathic perception of the mental state of another person.

CR (CRITICAL RATIO): A measure to determine whether or not the observed deviation is significantly greater than the expected random fluctuation about the average. The CR is obtained by dividing the observed deviation by the standard deviation. (The probability of a given CR may be obtained by consulting tables of the probability integral, such as Pearson's.)

CR OF THE DIFFERENCE: The observed difference between the average scores of two samples of data divided by the standard deviation of the difference.

DECK: Twenty-five ESP cards.

CLOSED DECK: An ESP deck composed of five each of the five symbols.

OPEN DECK: An ESP deck made up of the ESP symbols selected in random order.

DEVIATION: The amount an observed number of hits or an average score varies from the mean chance expectation or chance average. A deviation may be total (for a series of runs) or average (per run).

DIE THROW: The elementary unit of data in a dice-throwing test represented by the throwing and reading of a single die, no matter how many dice are thrown at a time.

DISPLACEMENT: ESP responses to targets other than those for which the calls were intended.

BACKWARD DISPLACEMENT: ESP responses to targets preceding the assigned targets (the ones for which they were intended). Displacement to the targets

one, two, three, etc. places preceding the assigned target are designated as (−1), (−2), (−3), etc.

FORWARD DISPLACEMENT: ESP responses to targets coming later than the assigned targets. Displacement to the targets one, two, three, etc. places after the assigned target are designated as (+1), (+2), (+3), etc.

DT (Down Through): The clairvoyance technique in which the cards are called down through the deck before any are removed or checked.

EMPIRICAL CONTROL: An experiment which wholly or partially follows the main experiment with the exception that the conditions are designed to exclude the possibility of the hypothesis being tested.

ESP (EXTRASENSORY PERCEPTION): Response to an external event not presented to any known sense.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, three parallel wavy lines (called "waves"), square, plus.

ESP SYMBOLS: See plate opposite page 1, this JOURNAL, Vol. 1, March, 1937.

EXPECTATION; see CHANCE.

EXTRACHANCE: Not due to chance alone.

FREE MATERIAL: The stimulus objects of experiments in which an unlimited or unspecified range of stimulus objects is employed (as contrasted with methods such as card-calling in which the subject knows that the stimulus object is one of a known range).

FREE RESPONSE METHOD: An ESP test method in which a relatively unlimited range of stimulus objects is used and in which the subject is instructed to express freely as his response any range of thoughts and ideas.

GESP (GENERAL EXTRASENSORY PERCEPTION): A technique designed to test the occurrence of extrasensory perception, permitting either telepathy or clairvoyance or both to operate.

HIGH-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling eight or more.

LOW-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling six or less.

MEAN CHANCE EXPECTATION; see CHANCE.

P (PROBABILITY): A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARAPSYCHOLOGY: A division of psychology dealing with those psychical effects which appear not to fall within the scope of what is at present recognized law.

PERCIPIENT: The person who makes the calls in an ESP test.

PK (PSYCHOKINESIS): The direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation.

PRECOGNITION: Cognition of a future event which could not be known through rational inference.

PREFERENTIAL MATCHING: A method of scoring free responses. A judge ranks the stimulus objects (usually in sets of four) with respect to their similarity to, or association with, each response; and/or he ranks the responses with respect to their similarity to, or association with, each stimulus object.

PSI: A general term to identify personal factors or processes in nature which transcend accepted laws. It approximates the popular use of the word "psychic" and the technical one, "parapsychical."

PSI PHENOMENA: Occurrences which result from the operation of psi. They include the phenomena of both ESP (including precognition) and PK.

QD (QUARTER DISTRIBUTION): The distribution of hits in the record page (or in a logical subdivision thereof, such as the set or the half-set) as found in the four equal quarters formed by dividing the selected unit horizontally and vertically.

RUN: A group of trials, usually the successive calling of a deck of 25 ESP cards or symbols. In PK tests, 24 single die throws regardless of the number of dice thrown at the same time.

SCORE: The number of hits made in one run.

TOTAL SCORE: Total of scores made in a given number of runs.

AVERAGE SCORE: Total score divided by number of runs.

SD (STANDARD DEVIATION): Usually the theoretical root mean square of the deviations. It is obtained from the formula $\sqrt{npq}$, in which n is the number of single trials, p the probability of success per trial, and q the probability of failure. (For ESP cards, SD $= 2\sqrt{\text{no. of runs.}}$) Sometimes the SD used is that estimated from the observed variability in the scores.

SD OF THE DIFFERENCE: For both ESP cards and PK tests using dice, the SD of the difference is equal to $\sigma_s \sqrt{1/R_1 + 1/R_2}$ where σ_s is the SD of a single run and R_1 and R_2 are the number of the runs in the respective samples compared. This gives the SD of the difference for run score averages.

SERIES: Several runs or experimental sessions that are grouped in accordance with a stated principle.

SESSION: A unit of an ESP or PK experiment comprising all the trials of one test occasion.

SET: A subdivision of the record page serving as a scoring unit for a consecutive group of trials, usually for the same target.

SEVENS TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling seven.

SIGNIFICANCE: A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. The criterion commonly used in this JOURNAL is a probability value of .02 or less, or a deviation in either direction such that the CR is 2.33 or greater.

SINGLES TEST: A PK technique in which the aim of the subject is to try to influence dice to fall with a specified face up.

STIMULUS OBJECT: The ESP card or drawing or other object, some identifying characteristic of which is to be apprehended by the percipient.

TARGET: In ESP tests, the stimulus object; or in telepathy, the mental state of the agent. In PK tests, the faces of the die (or combination of faces) which the subject attempts to bring up in the act of throwing.

TARGET CARD: The card which the percipient is attempting to perceive (i.e., to identify or otherwise indicate a knowledge of).

SUBJECT: The person who is experimented upon. In ESP tests, most commonly the percipient (though also the agent in GESP and telepathy). In PK tests, any individual whose task it is to influence the objects thrown.

TARGET DECK: The deck of cards the order of which the subject is attempting to identify.

TARGET FACE: The face on the die which the subject tries to turn up as a consequence of direct mental action.

TELEPATHY: Extrasensory perception of the mental activities of another person. It does not include the clairvoyant perception of objective events.

TRIAL: In ESP tests, a single attempt to identify a stimulus object. In PK tests a single throw of the dice or other objects thrown.

but equally science must not shrink from entering the territories of mind and spirit, however difficult and scientifically unorthodox they may appear.

. . . . . .

But man . . . is not merely a material mechanism. He has a mental aspect and his intellectual, emotional, and spiritual properties are certainly no less important than his material nature and needs. And they too can be approached in a scientific way. The scientific approach need not and cannot concern itself with metaphysical questions about the greater "reality" of mind or matter, or with theological speculations about the soul. But it can and must be naturalistic, and ask what properties are possessed by the human mind and spirit, and what functions they play in human life. It can attempt a survey of man's non-material as well as of his material needs, and of the possible methods of meeting them.

—Julian Huxley, "Population and Human Destiny,"
Harper's Magazine, September, 1950.

A PREFACE TO PARAPSYCHOLOGY?

The belief in man—man as an individual . . . free to think as he pleases and say as he thinks . . . is not easy to articulate . . . in such a world as modern physics has revealed to us. And yet, without a new and convincing articulation of [this] belief . . . civilization as we have known it—civilization crowned by science and by art—civilization carried forward by the creative spirit and the inquiring mind—would be unthinkable. It would disappear behind the blank conformist institutionalism which, not only in Russia, not only in Europe, threatens to board up the outward windows of the world.

—Archibald MacLeish, "The American State of Mind,"
The American Scholar, Autumn, 1950

Earlier views of human destiny have never had a scientific basis. They have for the most part been based on religious doctrines, such as those of karma and reincarnation in the East, or Christian theology in the West. . . .

Today, the time is overdue to use scientific method . . . so that our approach can be both scientific and unitary. By a scientific approach I mean an approach based on knowledge and the appeal to fact, as against one based on mere authority or alleged revelation, or derived only from logic and a priori argumentation, in either case leading to dogmatic conclusions of one sort or another. And by a unitary approach, I mean that we must avoid like the devil all the old dualisms—of soul versus body, spirit versus matter, religion versus science: they are all rooted in ignorance, and therefore bound to be misleading. The highest spiritual manifestations of man's life are just as natural as are material aspects like digestion or locomotion. There must be no territory forbidden to science, and exclusively reserved to religion or philosophy;

(Continued on inside back cover)